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LOGIC OF CHANCE.

THE

LOGIC OF CHANCE

AN ESSAY

ON THE FOUNDATIONS AND PROVINCE OF THE THEORY OF PROBABILITY,

WITH ESPECIAL REFERENCE TO ITS APPLICATION TO MORAL AND SOCIAL SCIENCE.

BY

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"So careful of the type she seems
So careless of the single life."

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PREFACE.

ANY work on Probability by a Cambridge man will be so likely to have its scope and its general treatment of the subject prejudged, that it may be well to state at the outset that the following Essay is in no sense mathematical. Not only, to quote a common but often delusive assurance, will 'no knowledge of mathematics beyond the simple rules of Arithmetic' be required to understand these pages, but it is not intended that any such knowledge should be acquired by the process of reading them. On the two or three occasions on which algebraical formulæ occur they will not be found to form any essential part of the text.

The science of Probability occupies at present a somewhat anomalous position. It is impossible, I think, not to observe in it some of the marks and consequent disadvantages of a *sectional* study. By a small body of ardent students it has been cultivated with great assiduity, and the results

they have obtained will always be reckoned among the most extraordinary products of mathematical genius. But by the general body of thinking men its principles seem to be regarded with indifference or suspicion. Such persons may admire the ingenuity displayed, and be struck with the profundity of many of the calculations, but there seems to them, if I may so express it, an *unreality* about the whole treatment of the subject. To many persons the mention of Probability suggests little else than the notion of a set of rules, very ingenious and profound rules no doubt, with which mathematicians amuse themselves by setting and solving puzzles.

It must be admitted that some ground has been given for such an opinion. The examples commonly selected by writers on the subject, though very well adapted to illustrate its rules, are for the most part of a special and peculiar character, such as those relating to dice and cards. When they have searched for illustrations drawn from the practical business of life, they have generally, by a strange fatality, hit upon just the sort of instances which, as I shall endeavour to show hereafter, are among the very worst that could be chosen for the purpose. It is scarcely possible for any unprejudiced person to read what has been written about the credibility

of witnesses by eminent writers, without his experiencing an invincible distrust of the principles which they adopt. To say that the rules of evidence sometimes given by such writers are broken in practice, would scarcely be correct; for the rules are of a kind which generally defies any attempt to appeal to them in practice.

This supposed want of harmony between Probability and other branches of Philosophy is perfectly erroneous. It arises from the belief that Probability is a branch of mathematics trying to intrude itself on to ground which does not belong to it. I shall endeavour to show that this belief is unfounded. To answer correctly the sort of questions to which the science introduces us does generally demand some knowledge of mathematics, often a great knowledge, but the discussion of the fundamental principles on which the rules are based does not necessarily require any such qualification. Questions might arise in other sciences, in Geology, for example, which could only be answered by the aid of arithmetical calculations. In such a case any one would admit that the arithmetic was extraneous and accidental. However many questions of this kind there might be here, those persons who do not care to work out special results for themselves might still have an accurate knowledge of the

principles of the science, and even considerable acquaintance with the details of it. The same holds true in Probability ; its connexion with mathematics, though certainly far closer than that of most other sciences, is still of much the same kind. It is principally when we wish to work out results for ourselves that mathematical knowledge is required; without such knowledge the student may still have a firm grasp of the principles and even see his way to many of the derivative results.

The opinion that Probability, instead of being a branch of the general science of evidence which happens to make much use of mathematics, *is* a portion of mathematics, erroneous as it is, has yet been very disadvantageous to the science in several ways. Students of Philosophy in general have thence conceived a prejudice against Probability, which has for the most part deterred them from examining it. As soon as a subject comes to be considered 'mathematical' its claims seem generally, by the mass of readers, to be either on the one hand scouted or at least courteously rejected, or on the other to be blindly accepted with all their assumed consequences. Of impartial and liberal criticism it obtains little or nothing.

The consequences of this state of things have been, I think, disastrous to the students them-

selves of Probability. No science can safely be abandoned entirely to its own devotees. Its details of course can only be studied by those who make it their special occupation, but its general principles are sure to be cramped if it is not exposed occasionally to the free criticism of those whose main culture has been of a more general character. Probability has been very much abandoned to mathematicians, who as mathematicians have generally been unwilling to treat it thoroughly. They have worked out its results, it is true, with wonderful acuteness, and the greatest ingenuity has been shown in solving various problems that arose, and deducing subordinate rules. And this was all that they could in fairness be expected to do. Any subject which has been discussed by such men as Laplace and Poisson, and on which they have exhausted all their powers of analysis, could not fail to be profoundly treated, so far as it fell within their province. But from this province the real principles of the science have generally been excluded, or so meagrely discussed that they had better have been omitted altogether. Treating the subject as mathematicians such writers have naturally taken it up at the point where their mathematics would best come into play, and that of course has not been at the foundations. In the works of most

writers upon the subject we should search in vain for anything like a critical discussion of the fundamental principles upon which its rules rest, the class of inquiries to which it is most properly applicable, or the relation it bears to Logic and the general rules of inductive evidence. Even in the essay of Laplace, a work commonly regarded as the principal philosophical text-book on the subject, the definition at the outset includes the very conception of Probability which it undertakes to explain. In the hands of less systematic writers, especially amongst those who have treated the subject in a popular way, such confusion becomes far more serious. One proof only need be given to show the utter vagueness and uncertainty with which the foundations of the science are frequently conceived. In different books, of which some refer to the others as authorities,—to a certain extent indeed in the same books,—we shall find Probability spoken of, sometimes as a property of mind, namely, the intensity of the belief with which we entertain a proposition; sometimes as something external to us which measures this intensity; sometimes as an abstract number, namely, a numerical fraction.

This want of precision as to ultimate principles is perfectly compatible here, as it is in the departments of Morals and Politics, with a general

agreement on processes and results. But it is, to say the least, unphilosophical, and denotes a state of things in which positive error is always liable to arise whenever the process of controversy forces us to appeal to the foundations of the science. I shall endeavour to show that this has actually been the case, and that confusion and perplexity have been thus introduced into some of the most important controversies agitated at the present day.

With regard to the remarks in the last few paragraphs, prominent exceptions must be made in the case of two recent works at least*. The first of these is Professor De Morgan's *Formal Logic*. He has there given an investigation into the foundations of Probability as conceived by him, and nothing can be more complete and precise than his statement of principles, and his deductions from them. If I could at all agree with these principles there would have been no necessity for the following essay, as I could not hope to add anything to their foundation, and should be far indeed from rivalling his lucid statement of

* I am here speaking, of course, of those only who have expressly treated of the foundations of the science. Mr Todhunter's great work on the *History of the Theory of Probability* being, as the name denotes, mainly historical, such enquiries have not directly fallen within his province.

them. But in his scheme Probability is regarded very much from the conceptualist point of view; as stated in the preface, he considers that Probability is concerned with formal inferences in which the premises are entertained with a conviction short of absolute certainty. With this view I cannot agree. As I have given a detailed criticism of some points of his scheme in one of the following Chapters, and shall have occasion frequently to refer to his work, I need say no more about it here. The other work to which I refer is the profound *Laws of Thought* of the late Professor Boole, to which somewhat similar remarks may in part be applied. Owing however to his peculiar treatment of the subject I have scarcely anywhere come into contact with any of his expressed opinions.

The view of the province of Probability adopted in this Essay differs so radically from that of most other writers on the subject, and especially from that of those just referred to, that I have thought it better, as regards details, to avoid all criticism of the opinions of others, except where conflict was unavoidable. With regard to that radical difference itself Bacon's remark applies, behind which I must shelter myself from any charge of presumption,—"Quod ad universalem istam reprehensionem attinet, certissimum vere est

rem reputanti, eam et magis probabilem esse et magis modestam, quam si facta fuisset ex parte."

Almost the only writer who seems to me to have expressed a just view of the nature and foundation of the rules of Probability is Mr Mill, in his *System of Logic*. His treatment of the subject is however very brief, and a considerable portion of the space which he has devoted to it is occupied by the discussion of one or two special examples. There are moreover some errors, as it seems to me, in what he has written, which will be referred to in some of the following chapters.

The reference to the work just mentioned will serve to convey a general idea of the view of Probability adopted in this Essay. With what may be called the Material view of Logic as opposed to the Formal or Conceptualist,—with that which regards it as taking cognisance of laws of things and not of the laws of our own minds in thinking about things,—I am in entire accordance. Of the province of Logic, regarded from this point of view, and under its widest aspect, Probability may, in my opinion, be considered to be a portion. The principal objects of this Essay are to ascertain how great a portion it comprises, where we are to draw the boundary between it and the contiguous branches of the general science of evidence, what are the ultimate foundations upon which its rules

rest, what the nature of the evidence they are capable of affording, and to what class of subjects they may most fitly be applied. That the science of Probability, on this view of it, contains something more important than the results of a system of mathematical assumptions, is obvious. I am convinced moreover that it can and ought to be rendered both interesting and intelligible to ordinary readers who have any taste for philosophy. In other words, if the large and growing body of readers who can find pleasure in the study of books like Mill's *Logic* and Whewell's *Inductive Sciences*, turn with aversion from a work on Probability, the cause in the latter case must lie either in the view of the subject or in the manner and style of the book.

The general design of the following Essay, as a special treatise on Probability, is, I think, original. Hence, probably, many errors will be detected in it, and most certainly many omissions and imperfections of treatment. Some of these might perhaps have been guarded against by delaying publication, but however much one might be tempted to delay for one's own reputation and personal feeling, it is very doubtful whether the interests of science are generally best advanced by such a course. Provided always that the principal ideas and their connection with one another

have been thoroughly thought out, it seems to me that one had better bring them out at once for others to look at and see what they are worth. Hostile criticism is a rough but very efficacious way of finding out errors, and a few months of such contact with others may in the end do more good than would be attained by years of solitary reflection. I only hope that those who may detect errors and inconsistencies in the following pages will not too readily conclude that they are a sign of crude thought or over hasty publication. No one, until he has actually made the attempt, can conceive the prodigious difficulty of thinking and writing with perfect consistency upon a subject which has been already treated by men his superiors in ability and knowledge, but which they have discussed from a very different point of view. Under such circumstances it is almost vain to hope that he can have entirely escaped from what he is bound in reason to regard as their misleading influence.

I take this opportunity of thanking several friends, amongst whom I must especially mention Mr Todhunter, of St John's College, and Mr H. Sidgwick, of Trinity College, for the trouble they have kindly taken in looking over the proof-sheets, whilst this work was passing through the press. To the former in particular my thanks are

due for thus adding to the obligations which I, as an old pupil, already owed him, by taking an amount of trouble, in making suggestions and corrections for the benefit of another, which few would care to take for anything but a work of their own. His extensive knowledge of the subject, and his extremely accurate judgment, render the service he has thus afforded me of the greatest possible value.

J. V.

Gonville and Caius College.
September, 1866.

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ERRATA.

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THE LOGIC OF CHANCE.

CHAPTER I.

ON A CERTAIN KIND OF SERIES AS THE FOUNDATION OF PROBABILITY.

§ 1. It is not generally easy to give a clear definition of a science at the outset, so as to set its scope and province before the reader in a few words. In the case of those sciences which are more immediately and directly concerned with objects, this difficulty is not indeed so serious. If the reader is already familiar with the objects, a simple reference to them will give him a tolerably accurate idea of the direction and nature of his studies. Even if he be not familiar with them, they will still be often connected and associated in his mind by a name, and the mere statement of the name will convey a fair amount of preliminary information. This is more or less the case with many of the natural sciences; we can often tell the reader beforehand exactly what he is going to study. But when a science is concerned, not so much with objects directly, as with processes and laws, or when it takes for the subject of its enquiry some comparatively obscure feature drawn from phenomena

which have little or nothing else in common, the difficulty of giving preliminary information becomes greater. Recognised classes of objects have then to be disregarded and even broken up, and an entirely novel arrangement of the objects to be made. In such cases it is the study of the science that first gives the science unity, for till it is studied the objects with which it is concerned were probably never thought of together. Here a definition cannot be given at the outset, and the process of obtaining it may become somewhat laborious.

The science of Probability, at least on the view taken of it in the following pages, is of this latter description. The reader who is at present unacquainted with it cannot be at once informed of its scope by a reference to objects with which he is already familiar. He must be taken in hand, as it were, and shown the objects before he will know them. To do this will be our first task.

§ 2. In studying Nature, in any form, we are continually coming into possession of information which we sum up in general propositions. Now in very many cases these general propositions are neither more nor less certain and accurate than the details which they embrace and of which they are composed. I am assuming at present that the truth of our generalizations is not disputed; as a matter of fact these generalizations may rest on weak evidence, or they

may be uncertain from their being widely extended by Induction; what I mean is, that when we resolve them into their component parts we have precisely the same assurance of the truth of the details as we have of that of the whole. When I know, for instance, that cows ruminate, I feel just as certain that any particular cow or cows ruminate as that the whole class does. I may be right or wrong in my original statement, and I may have obtained it by any conceivable mode; but whatever the value of the general proposition may be, that of the particulars is neither greater nor less. If one of these 'immediate inferences' is justified at all, it will be always right.

But it is by no means necessary that this should be the case. There is a class of immediate inferences, unrecognized indeed in Logic, but constantly drawn in practice, of which the characteristic is, that as they increase in particularity they diminish in certainty. Let me assume that I am told that *some* cows ruminate; I cannot infer logically from this that any particular cow does so, though I should feel some way removed from absolute disbelief or even indifference on the subject; but if I saw a herd of cows I should feel more sure that some of them were ruminant than I did of the single cow, and my assurance would increase with the numbers of the herd about which I had to form an opinion. Here then we have a class of objects as to the individuals of which we feel quite

in uncertainty, whilst as we embrace larger numbers in our assertions we attach greater weight to our inferences. It is with such classes of objects and such inferences that the science of Probability is concerned.

§ 3. In the foregoing remarks, which are intended to be purely preliminary, I have not been able altogether to avoid some reference to a subjective element, viz. the degree of our certainty or belief about the things which we are supposed to contemplate. The reader may be aware that by some writers this element is regarded as the subject-matter of the science. Hence it will have to be discussed in a future chapter. As however I do not at all agree with the opinion of the writers just mentioned, I shall make no further allusion to it here, but pass on at once to a more minute investigation of that distinctive characteristic of certain classes of things which was introduced in the last section.

In these classes of things, which are those with which Probability is concerned, the fundamental conception which the reader has to fix in his mind as clearly as possible, is, I take it, that of a series. But it is a series of a peculiar kind, one of which I can see no better compendious description than that which is given by the statement that it combines individual irregularity with aggregate regularity. This is a statement which will probably need some explanation. Let us recur to an example of the kind already

alluded to, selecting one which shall be in accordance with experience. Some cows will not suckle their young. Now if this proposition is to be regarded as a purely indefinite or, as it would be termed in logic, particular proposition, no doubt the notion of a series does not obviously present itself in connection with it. It contains a statement about a certain unknown proportion of the whole, and that is all. But it is not with these purely indefinite propositions that we shall be concerned. Let us suppose the statement, on the contrary, to be of a numerical character, and to refer to a given proportion of the whole, and we shall then find it difficult to exclude the notion of a series. We shall find it, I think, impossible to do so as soon as we set before us the aim of obtaining accurate or even moderately correct inferences. What, for instance, is the meaning of the statement that one cow in ten fails to suckle its young? It certainly does not declare that in any given herd of, say twenty, we shall find just two that fail: whatever might be the strict meaning of the words, this is not the import of the statement. It rather contemplates our examination of a large number, of a long succession of instances, and states that in such a succession we shall find a numerical proportion, not indeed accurate at first, but which tends in the long run to become accurate. In every kind of example with which we shall be concerned we shall find, I think, this refer-

ence to a large number or succession of objects, or, as I shall term it, *series* of them.

A few additional examples may serve to make this plain.

Let us suppose that we toss up a penny a great many times; the results of the successive throws may be conceived to form a series. The separate throws of this series seem to occur in utter disorder; it is this disorder which causes our uncertainty about them. Sometimes head comes, sometimes tail comes; sometimes there is a repetition of the same face, sometimes not. So long as we confine our observation to a few throws at a time, the series seems to be simply chaotic. But when we consider the result of a long succession we find a marked distinction; a kind of order begins gradually to emerge, and at last assumes a distinct and striking aspect. We find in this case that the heads and tails occur in about equal numbers, that similar repetitions of different faces do so also, &c. In a word, notwithstanding the individual disorder, an aggregate order begins to prevail. So again if we are examining the length of human life, the different lives which fall under our notice compose a series presenting the same features. The length of a single life is proverbially uncertain, but the average duration of a batch of lives is becoming in an almost equal degree proverbially certain. The larger the number we take out of any mixed crowd,

the clearer become the symptoms of order, the more nearly will the average length of each selected class be the same. These few cases will serve as simple examples of a property of things which can be traced almost everywhere, to a greater or less extent, throughout the whole field of our experience. Fires, shipwrecks, yields of harvest, births, marriages, suicides; it scarcely seems to matter what feature we single out for observation. The irregularity of the single instances diminishes when we take a large number, and at last seems for all practical purposes to disappear.

§ 4. In speaking as above of events or things as to the details of which we know little or nothing, it is not of course implied that our ignorance about them is complete and universal, or, what comes to the same thing, that irregularity may be observed in all their qualities. All that is meant is that there are *some* qualities or marks in them, the existence of which we are not able to predicate in the individuals. With regard to all their other qualities there may be the utmost uniformity, and consequently the most complete certainty. The irregularity in the length of human life is notorious, but no one doubts the existence of a heart and brains in any person whom he happens to meet. And even in the qualities in which the irregularity is observed, there are often, indeed generally, positive limits within which it will be

found to be confined. No person, for instance, can calculate what may be the length of any particular life, but we feel perfectly certain that it will not stretch out to 150 years. The irregularity of the individual instances is only shewn in certain respects, as e. g. the length of the life, and even in these it has its limits. The same remark will apply to most of the other examples with which we shall be concerned. The disorder in fact is not universal and infinite, it only prevails in certain directions and up to a certain point.

§ 5. In speaking as above of a series, it will hardly be necessary to point out that we do not imply that the objects themselves which compose the series must occur successively in time; the series may be formed simply by their coming in succession under our notice, which as a matter of fact they may do in any order whatever. A register of mortality, for instance, may be made up of deaths which took place simultaneously or successively; or we might if we pleased arrange the deaths in an order quite distinct from either of these. This is entirely a matter of indifference; in all these cases the series, for any purposes which we need take into account, may be regarded as being of precisely the same description. The objects, be it remembered, are given to us in nature; the order under which we view them is our own private arrangement. I mention this here simply by way of

caution, the meaning of this assertion will become more plain in the sequel.

§ 6. The reader will now have in his mind the conception of a series of things or events, of the individuals of which we know but little, whilst we find a continually increasing uniformity as we take larger numbers under our notice. This is definite enough to point out tolerably clearly the kind of things with which we have to deal, but it is not sufficiently definite for purposes of accurate thought. We must therefore attempt a somewhat closer analysis.

There are certain phrases so commonly adopted, as to have become part of the technical vocabulary of the subject, such as an 'event' and the 'way in which it can happen.' Thus the act of throwing a penny would be called an event, and the fact of its giving head or tail would be called the way in which the event happened. If we were discussing tables of mortality, the former term would denote the mere fact of death, the latter the age at which it occurred, or the way in which it was brought about, or whatever else in it might be the particular circumstance under discussion. This phraseology is very convenient, and I shall often make use of it, but without explanation it may lead to confusion. For in many cases the way in which the event happens is of such great relative importance, that according as it happens in one way or another the event would have a different

name, in other words, would or would not be the same event. The phrase therefore will have to be considerably stretched before it will conveniently cover all the cases to which we may have to apply it. If we were contemplating a series of human beings, male and female, it would sound odd to call their humanity an event, and their sex the way in which the event happened. If we recur however to any of the classes of objects already referred to, we may see our path towards obtaining a more accurate conception of what we want. It will easily be seen that in every one of them there is a mixture of similarity and dissimilarity; there is a series of events which have a certain number of features or attributes in common,—without this they would not be classed together. But there is also a distinction existing amongst them; a certain number of other attributes are to be found in some and are not to be found in others. In other words, the individuals which form the series are compound, each being made up of a collection of things or attributes; some of these things exist in all the members of the series, others are found in some only. So far there is nothing peculiar to the science of Probability; that in which the distinctive characteristic consists is this;—that the occasional attributes, as distinguished from the permanent, are found on an extended examination to exist *in a certain definite proportion of the whole number of cases.* We cannot tell in any given instance whether

they will be found or not, but as we go on examining more cases we find a growing uniformity. We find that the proportion of instances in which they are found to instances in which they are wanting, is gradually subject to less and less variation, and approaches continually towards some apparently fixed value.

The above is the most comprehensive form of description; as a matter of fact the groups will in many cases take a far simpler form, they may appear, e. g. simply as a succession of substances of the same kind, say cows, with or without an occasional attribute, say redness. I am using the word attribute, of course, in its widest sense, intending it to include every distinctive feature that can be observed in a thing, from essential qualities down to the merest accidents of time and place.

§ 7. On examining our series, therefore, we shall find that it may best be conceived, not as a succession of events happening in different ways, but as a succession of groups. These groups, on being analysed, are found in every case to be resolvable into collections of substances and attributes. That which gives its unity to the succession of groups is the fact of some of these substances or attributes being common to the whole succession; that which gives their distinction to the groups in the succession is the fact of some of them containing only a portion of these substances and

attributes, the other portion or portions being occasionally absent. So understood, I think our phraseology will embrace every class of subjects of which Probability can take account.

§ 8. It will be easily seen that the ordinary expression is included in the one adopted above. When the occasional attributes are unimportant the permanent ones are sufficient to fix and appropriate the name, the presence or absence of the others being simply denoted by some modification of the name or the addition of some predicate. We may therefore in all such cases speak of the collection of attributes as 'the event,'—the same event essentially, that is—only saying that *it* (so as to preserve its nominal identity) happens in different ways in the different cases. When the occasional attributes however are important, or compose the majority, this way of speaking becomes less appropriate; language is somewhat strained by our implying that two extremely different assemblages are in reality the same event, with a difference only in its mode of happening. The phrase is however a very convenient one, and with this caution against its being misunderstood, I shall frequently make use of it.

§ 9. A series of the above-mentioned kind is, I apprehend, the ultimate basis upon which all the rules of Probability must be based. It is essential to a clear comprehension of the subject to have carried our analysis up to this point, but any attempt at further

analysis into the intimate nature of the events composing the series, is not required. It is not necessary, for instance, to form any opinion upon the questions discussed in metaphysics as to the independent existence of substances. We have discovered, on examination, a series composed of groups of substances and attributes, or of attributes alone. At such a series we stop, and thence investigate our rules of evidence; into what these substances or attributes would themselves be ultimately analysed it is no business of ours to enquire here.

§ 10. The stage then which we have now reached is that of having discovered a quantity of things (they prove to be groups on analysis) which are capable of being classified together, and are best regarded as a series. The distinctive peculiarity of this series is our finding in it an order, gradually emerging out of disorder, and showing in time a marked and unmistakeable uniformity. The impression which may possibly be derived from the description of such a series, and which the reader will probably already entertain if he have studied Probability before, is that the gradual evolution of this order is indefinite, and its approach therefore to perfection unlimited. And many of the examples commonly selected certainly tend to confirm such an impression. But in reference to the theory of the subject it is, I am convinced, an error, and one fraught with confusion.

The lines which have been prefixed as a motto to this work, "So careful of the type she seems, so careless of the single life," are soon after corrected by the assertion that the type itself, if we regard it for a long time, changes and then vanishes and is succeeded by others. So in Probability; that uniformity which is found in the long run, offering so great a contrast to the individual disorder, though durable is not everlasting. Keep on watching it long enough, and it will be found almost invariably to fluctuate, and in time may prove as utterly irreducible to rule, and therefore as incapable of prediction, as the individual cases themselves. The full bearing of this fact upon the theory of the subject, and upon certain common modes of calculation connected with it, will appear more fully in some of the following chapters; at present I shall confine myself to establishing and illustrating it.

Let us take, for example, the average duration of life. This, provided our data are sufficiently extensive, is known to be tolerably regular and uniform. This has been fully illustrated in the preceding sections, and is a truth indeed of which the popular mind has a tolerably clear grasp at the present day. But a very little consideration will show that there may be a superior as well as an inferior limit to the extent within which this uniformity can be observed. At the present time the average duration of life in

England may be, say thirty; but a century ago it was decidedly less; several centuries ago it was very much less; whilst if we possessed statistics referring to our early British ancestors we should probably find that there has been since that time a still more marked improvement. What may be the future tendency no man can say for certain. It may be, and we hope will be the case, that owing to sanitary and other improvements, the duration of life will go on increasing steadily; it is quite conceivable that it should do so without limit. On the other hand, this duration might gradually tend towards some fixed length. Or, again, it is perfectly possible that future generations might prefer a short and a merry life, and therefore reduce their average. All that I am concerned to indicate is, that this uniformity (as we have hitherto called it) has varied, and, under the influence of future eddies in opinion and practice, may vary still; and this to any extent, and with any degree of irregularity. To borrow a term from Astronomy, we find our uniformity subject to what might be called an irregular *secular* variation.

§ 11. The above is a fair typical instance. If we had taken a less simple feature than the length of life, or one less closely connected with what may be called the great permanent uniformities of nature, we should have found the peculiarity under notice exhibited in a far more striking degree. The deaths from small-

pox, for example, or the instances of duelling or accusations of witchcraft, if examined during a few successive years, would have shown a very tolerable degree of uniformity. But this uniformity has risen probably from zero; after various and very great fluctuations seems tending towards zero again; and may, for anything we know, undergo still greater fluctuations in future. Now these examples I consider to be only extreme ones, and not such very extreme ones, of what is the almost universal rule in nature. I shall endeavour to show that even the few apparent exceptions, such as the proportions between male and female births, &c., may not be, and probably in reality are not, exceptions. A type that is persistent and invariable is scarcely to be found in nature. The full import of this conclusion will be seen in a future chapter. I would only call attention here to the important inference that, although statistics are notoriously of no value unless they are in sufficient numbers, yet it does not follow but that we may have too many of them. If they are made too extensive, they may again fall short, at least for any particular time or place, of their greatest attainable accuracy.

§ 12. These natural uniformities then are found at length to be subject to fluctuation. Now contrast with them any of the uniformities afforded by games of chance; these latter seem to show no trace of secular fluctuation, however long we may continue

our examination of them. Some criticism will be offered, in the course of the next chapter, upon some of the common attempts to prove *à priori* that there must be this fixity in the uniformity, but of its existence there can scarcely be much doubt. Pence give heads and tails alternately now, as they did when they were first tossed, and as we believe they will continue to do so long as the present order of things continues. The fixity of these uniformities may not be as absolute as is commonly supposed, but no amount of experience which we need take into account is likely to shake them. Whereas natural uniformities at length fluctuate, those afforded by games of chance seem fixed for ever.

§ 13. Here then are series apparently of two different kinds. They are alike in their initial irregularity, alike in their subsequent regularity; it is in what we may term their ultimate form that they begin to diverge from one another. The one tends without any permanent variation towards a fixed numerical proportion in its uniformity; in the other the uniformity is found at last to fluctuate, and to fluctuate, it may be, in a manner utterly irreducible to rule.

§ 14. As this chapter is intended to be simply explanatory and illustrative of the foundations of the science, I may remark here (what will receive its subsequent justification) that it is in the case of series of the former kind only that we are able to make any-

thing which can be interpreted into strict scientific inferences. We shall be able however to see the kind and extent of error that would be committed if in any example we were to substitute an imaginary series of the former kind for any actual series of the latter kind which experience may present to us. The two series are of course to be alike in all respects, except that the variable uniformity has been replaced by a fixed one. The difference then between them would not appear in the initial stage, for in that stage the distinctive characteristics of the series of Probability are not apparent; nor would it appear in the subsequent stage, for the real variability of the uniformity has not for some time scope to make itself perceived. It would only be in what we have called the ultimate stage, when we suppose the series to extend for a very long time, that the difference would begin to make itself felt. The numbers of persons, for example, who die each year at the age of six months are, when examined on a small scale, utterly irregular; they become however regular when examined on a larger scale; but if we continued our observation for a very great length of time, or over a very great extent of country, we should find this regularity itself changing in an irregular way. The substitution just mentioned is equivalent to saying, Let us assume that the regularity is fixed and permanent. It will appear in a future chapter that such a substitution has to be

made in very many examples before they can become fit subjects for strict scientific inference.

§ 15. As the theory of Probability is almost understood when the foregoing conceptions are fully grasped, I will add, for additional clearness, a physical illustration. It will be worth while to work this out rather fully, so as to make it serve once for all as a sort of standard illustration to refer back to. I might indeed call it an analogy rather than an illustration, as it is in reality an application of the same principle to geometry instead of to arithmetic. Let the reader then picture to himself a mountainous country, the surface of which is all broken up into crag and boulder and gully. If we wished to measure the surface of this country we should find mathematics of very little use directly; we could only give rough guesses or succeed at last by the tedious process of measuring almost every patch of a few square yards separately. But in parts of it—at the bottom of long broad valleys for example—we should find the features of the country altogether altered. Here too (which it is very important to mark) there would be roughness and irregularity of surface when we confined our view to a small spot; to a short-sighted man it would still appear as if he were standing in the midst of a chaos; but when we took a broader view we should discover a regularity that in contrast with the broken hill-side would seem almost startling. The greater the extent

of country over which we cast our eye the more perfect would appear this regularity, till in the far distance the soil seemed smoothed down as though with a spirit-level. Such a characteristic as this at once enables us to make use of mathematics: if the bottom of the valley be a plane it must possess the properties of a plane, and hence the work of mensuration would become easy. It is not that we have made any arbitrary assumptions; in examining the face of nature we have found that a striking difference does in fact exist between one part and the other, by which the principles of geometry are enabled to serve our purpose by a simple and immediate application. In other words, although there is disorder over any very small space, and disorder again of another kind over a very large space (for then we may get out of the plain), there is between these extremes an extent of order. The discovery of this order redeems so much from the territory of ignorance, and we apply our geometrical rules to it at once.

Very similar to this is the case with Probability. When we get into statistics (supposing that sufficient scope is afforded for them) we step, as it were, from the rough hill-side on to the level of the plain. Here too we may for long have observed no regularity; but why? because we had kept our eyes too much fixed on the narrow spot where we were standing. The first distant view we thought of taking would suggest

the idea of uniformity, and the first measurements we made would verify it. In each case, in the midst of the apparent chaos of nature—a chaos however having its own rules, and in which strict inferences of other kinds might be drawn—we have found a certain portion of ground clearly separated off by its mathematical character; in the former the principles of geometry come to our help, and in the latter those of arithmetic.

§ 16. The reader's attention is especially directed to the following considerations, to which from their importance we shall have to be continually referring.

1. Let it be observed that experience alone can determine the extent over which our mathematics will apply. This extent is in no other sense a matter of demonstration. We must look for ourselves to see where the valley begins and ends, we must trust to observation to determine how far statistical regularity can be reckoned upon. We may make of course what use we can of the principles of inductive reasoning and analogy to aid us; but our conclusions ultimately rest entirely on experience, and are subject at any time to be tested and revised by specific experience.

2. In the next place it must be remarked that, although the natural phenomena themselves are of limited extent, the mathematics we apply to them recognize no limitation. The bottom of the valley coincides with a finite portion of a certain horizontal

plane, but that plane, of course, may be conceived as extending indefinitely (retaining all its properties) in every direction. Now in applying the properties of the plane to measure the valley, or to make other inferences about it, we must be very careful not to trespass beyond the limits which experience will justify; in other words, to take no more of the infinite plane than is wanted for the finite valley. In a case like this, where we can form a visual representation of an object, we are scarcely likely to fall into such a mistake; the distinctness with which such representations can be made was my reason for choosing the illustration; but a precisely similar fallacy is of perpetual occurrence in Probability. It is known, for instance, that about 250 persons annually commit suicide in London. Now if any one were to conclude from this that they would continue to do so, and that therefore all the sanctions of law and religion were vain to prevent them, he would be concluding, as it seems to me, that the valley must extend indefinitely in all directions. All that Probability, or statistics, can say is that *if* 250 commit suicide certain inferences may be drawn; but within what limits of time the number will remain the same it cannot give a hint; this must be decided by a far more complex process of experience and induction. The same criticism will apply to a very great number of the inferences drawn by social and political writers from the

statistics they have collected. The reader cannot have it too forcibly impressed upon him that the process of inference is as follows;—In examining the series of statistics which arise out of any of these natural uniformities we should generally find that as a matter of fact the series tend at length to lose their regularity. But this will not suit our purpose. What we do therefore (*vide* § 14) is to make a substitution, and employ instead a series which shall be regular throughout. It is by this substituted series that we do in reality make our inferences, just as in our illustration the real basis of our calculations is the imaginary plane. The validity of the inferences obtained clearly depends upon there being a close agreement between the substituted series and the real one; whenever the two diverge at all widely we are liable to fall into error. The very fact of the series we employ being so unlimited in its uniformity makes it more enticing to remain in it; an occasional appeal to experience therefore is necessary to test and control our conclusions. We use our mathematics, in fact, as a sort of rail-road; we quit a toilsome and impracticable path and are whirled along at our ease, often through a dark tunnel of symbols; but we must bear in mind that we have to get out again; if we do not keep a sharp look out we may be carried far beyond our destination.

3. A third consideration is that we are not to

look for any sudden break between the phenomena to which mathematics respectively can and cannot be applied. The distinction between the characteristics of the two classes, when these characteristics are in perfection, is undoubtedly striking, but they merge into one another very gradually. Between the valley and the hill-side there will be a sloping mass of débris insensibly sinking into the former; the valley itself also will not be perfectly true, and may gradually change its level. So in statistics. We shall, in the vast majority of instances, as already remarked, find that the numerical proportions, which by their persistence produce the uniformity, gradually change, and this to such an extent that the term uniformity at last becomes inappropriate. Hence the limits within which we collect our statistics are to a certain extent arbitrary; we must exercise our judgment in deciding where we will draw the line and what we will include within it.

§ 17. For additional clearness it may be worth while to point out to the reader what, in such an illustration as that above, would correspond to the distinction between the two kinds of series mentioned in a previous section. If, as we went along the valley, we found its level gradually change and its surface become uneven, it would belong to one of those series which possess a variable type; if, on the other hand, its level surface continued indefinitely, it would belong to one of those which possess a fixed type.

§ 18. In the foregoing remarks nothing has been said that would imply that the series presented a uniformity in more than one respect. As a matter of fact observation may detect any number of such uniformities, but as they are all of essentially the same kind the theory of the subject is unaffected. Thus, for example, in the succession of throws of a penny, we do not merely find that the heads are about as numerous as the tails, we find also that 'heads twice running' occurs about as frequently as 'tails twice running'; the same is the case with all other combinations. In the same way, if we were examining the tables of mortality of London, we should find not merely that about 1400 persons died every week, but that the numbers of each sex and every age showed some regularity, and preserved a definite proportion to one another. As all these different general uniformities present precisely the same characteristics, it is sufficient for my present purpose to call attention to the fact of their existence. Those who desire further information on this subject will find an abundance of interesting examples in a well-known work on Probability by M. Quetelet. I shall recur to the subject in the course of the next chapter.

CHAPTER II.

THE SERIES OF PROBABILITY ARE OBTAINED BY EXPERIMENT AND NOT *A PRIORI*, AND THEY OCCUR IN GROUPS.

§ 1. At the point which we have now reached we are supposed to be in possession of a series of a certain kind, lying at the bottom, as one may say, and which is the foundation on which the science of Probability is erected. The next two enquiries, which have to be made in order, are, how in any particular case such a series is to be obtained, and in what mode it is to be employed when we have obtained it.

§ 2. The answer to the former enquiry does not seem difficult. Experience is our sole guide. If we want to discover what is in reality a series of *things*, not a series of our own conceptions, we must appeal to the things themselves to obtain it, for we shall not find any help elsewhere. We cannot tell how many persons will be born or die in a year, or how many houses will be burned or ships wrecked, without actually counting them. In saying that experience is the sole guide, I mean of course experience supplemented by the aids that Inductive Logic can afford. When, for instance, we have found the series which represents the

numbers of persons who die in successive years, we have no hesitation in extending it some way into the future as well as the past. The justification of this procedure must be found in the ordinary canons of Induction. As a separate discussion will be given upon the connection between Probability and Induction, no more need be said on this subject here; but nothing will be found at variance with the assertion just made, that the series we employ are obtained simply from experience.

§ 3. If Probability were only concerned with the kind of events which in practice are commonly made subjects of insurance, probably no other view than the above would ever have obtained credence. But the fact of most of its examples having been chosen from such things as dice and cards has infected the whole science with an *à priori* tendency, which has biassed the minds of its followers in other applications.

An opinion prevails that in certain cases we are able to determine beforehand what the series will be, and this with such certainty that the real basis of our calculation is not the series itself, but some *à priori* conditions on which the series depends. As I consider this opinion to be erroneous in fact, and likely to cause confusion in the theory of the subject, I will proceed to a detailed examination of it; first in its stronghold of games of chance, and then in some of its other places of occasional resort. From the celebrity

of the writers who have maintained this view it could not be passed over in silence.

§ 4. Let us take a very simple example, that of tossing up a penny. Suppose that I am contemplating beforehand a succession of two throws; I clearly know that the only possible events are* H.H. H.T. T.H. T.T. So much is quite certain. We are moreover tolerably well convinced from experience that these events occur, in the long run, about equally often. This is admitted on all hands. But on the view against which I am contending it is asserted that we might have known the fact beforehand by principles which are applicable to an indefinite number of other and more complex cases. The form in which this view would generally be advanced is, that we are enabled to know beforehand that the four throws above mentioned are *equally likely*. This is asserted by almost every writer on the subject. To this I would ask, What is the meaning of the expression 'equally likely'? To such a question I think but two forms of reply are possible. The one of these would seek an explanation in the state of mind of the observer, the other would seek it in some characteristic of the things observed. (1) It might, for instance, be

* To those who are not acquainted with the common notation of mathematical works, I may say that H. H. is simply an abbreviated way of stating that the two successive throws of the penny give head; and the same with the other symbols.

said on the one hand that what is meant is that the two events contemplated are equally easy to imagine, or, more accurately, that our expectation or belief in their occurrence is equal. We could hardly be content with this reply, for the further enquiry would then be urged, On what grounds do we believe this? What are the characteristics of events of which our expectation is equal? If we consented to give an answer to this further enquiry we should be led, I think, to the second form of reply to be noticed directly; if we did not consent we should be admitting that Probability was only a portion of Psychology, confined therefore to considering states of mind and not the external events to which they referred. We should be ceasing to make it a science of inference about things. (2) In the other form of reply the explanation of the phrase in question would be sought, not in a state of mind but in a quality of the things contemplated. It might give the following as the meaning, viz. that the events really would occur with equal frequency in the long run. The ground of this assertion would probably be found in past experience, and it would doubtless be impossible so to frame the answer as to exclude the notion of our belief altogether. But still there is a broad distinction between seeking the equality in the amount of our belief, as before, and in the frequency of occurrence of the events, as here; I am convinced that this second form

of reply is the correct one, but it may easily be shewn that it is tantamount to abandoning the *à priori* theory, for it involves the admission that the real starting point of Probability is a sequence of events.

§ 5. For *can* the assertion in this reply be made *à priori?* Those who say it can, have never, I think, fairly faced the difficulties which meet them. For the moment we begin to enquire seriously whether the penny will really do what is expected of it, we shall find that restrictions have to be introduced. In the first place the penny must be an ideal one, with its sides equal and fair. This restriction is perfectly intelligible; the study of solid geometry has enabled me to idealize a penny into a circular or cylindrical lamina. This restriction is always admitted, but it is the only one that is admitted, although it is only one condition out of several that combine towards the result. But this is not sufficient; there are other conditions. We must also idealize the 'randomness' of the throwing of the penny, which is a process that one feels rather at a loss to know how to set about performing. This is no idle subtlety; for will it be asserted that the heads and tails would get their fair chances supposing that, in the act of throwing, I were always to start the same side uppermost? Scarcely, I think; the difference, slight as it is, might become at last appreciable. Or if I persisted in starting with the two sides upwards alternately, would the

long repetitions of the same side get *their* fair chance? Perhaps it will be replied that we are to think nothing at all about these matters, and all will come right. It may, and doubtless will, but this is falling back upon experience. For suppose, lastly, that the circumstances of nature or my bodily or mental constitution were such that the same side always *is* started uppermost; well, it will be replied, it would not then be a fair trial. I am convinced that if we press in this way for an answer to such enquiries, we shall find that these tacit restrictions on the *à priori* plan are really nothing else than a mode of securing an experimental result. They are only a way of saying, Let a series of actions be performed in such a way as to secure a sequence of a particular kind, viz. of the kind described in the last chapter.

The remarks above made will apply, of course, to most of the other common examples of chance; the throwing of dice, drawing of cards, of balls from bags, &c. In all these cases, if we scrutinize our language carefully, we shall find that the supposed *à priori* mode of stating the problem is little else than a compendious way of saying, Let means be taken for obtaining a given result. Since, then, it is upon this result that our inferences ultimately rest, it seems to me simpler and more philosophical to appeal to it at once as the groundwork of our science.

§ 6. It will be seen, by this time, how very nar-

row is the range of cases which the so-called *à priori* plan can be supposed to embrace. It is confined to games of chance, and can only be introduced there by the aid of many tacit restrictions. This alone would be conclusive against the theory of the subject being based upon it. The experimental plan, on the other hand, is of universal application. It would include the ordinary problems of games of chance, as well as those where the dice are loaded and the pence are not ideal, and also the indefinitely numerous applications of statistics to social phenomena and the facts of inanimate nature.

§ 7. I quite admit the advantages of the *à priori* plan where it can be used. Without it chance problems could scarcely be set in examinations, and the science would be deprived of a certain neatness and independence which the common mode of treatment confers upon it. Moreover, in many cases it would be a real hardship to be debarred from appealing to it. We are often enabled, by geometrical and other considerations, to ascertain with tolerable accuracy what kind of a sequence of events we may look for, at a time when we are either without specific experience or should find it tedious to obtain it. Against this as a practical measure not a word of objection can be raised; I am only contending that it is not the simplest and most consistent way of studying the theory of the subject. We may use an artifice for obtaining

the series, but it would be a great mistake to take anything but the series as the foundation of our rules.

§ 8. The *à priori* theory, in the form examined above, could scarcely have intruded itself into any other examples than those drawn from games of chance. But a doctrine, which is in reality little else than the same theory under a slightly disguised form, is very prevalent, and has been applied to truths of the most purely experimental character. This doctrine will be best introduced by a quotation from Laplace. After speaking of the irregularity and uncertainty of nature as it appears at first sight, he goes on to remark that when we look closer we begin to detect* "a striking regularity which seems to arise from design, and which some have considered a proof of Providence. But, on reflection, it is soon perceived that this regularity is nothing but the development of the respective probabilities of the simple events, which ought to occur more frequently according as they are more probable."

Now if this remark had been made about the succession of heads and tails in the throwing up of a penny, it would have been intelligible, though, as I have endeavoured to show, not philosophically correct. It would simply mean this: that the constitution of the body was such that we could anticipate what the

* Translated from Laplace, *Essai Philosophique*, p. 74.

result would be when it was treated in a certain way, and that experience, in the long run, would justify our anticipation. But applied as it is in a more general form to the facts of nature it seems altogether unmeaning. We will test it by taking what is perhaps one of the strongest conceivable instances in its support. Amidst the irregularity of individual births, we find that in the long run the male children are to the female in about the proportion of 106 to 100. Now when we are told that there is nothing in this but the "development of their respective probabilities," what is there in this sentence but a somewhat pretentious restatement of the fact just asserted? The probability *is* nothing but that proportion, and is derived from the statistics alone; in the above remark the attempt seems made to invert this process, and to derive the sequence of events from a numerical statement about the very events themselves.

§ 9. It will be said perhaps that by the probability above mentioned is meant, not the mere numerical proportion between the births, but some fact in our constitution upon which this proportion depends; that just as there was a relation of equality between the two sides of the penny, so there may be something in our bodies in the proportion of 106 to 100 which produces the statistical result.

When this something, whatever it might be, was discovered, the observed numbers might be supposed

capable of being determined beforehand*. Even if this were the case it is forgotten that there must be, in combination with such a cause, other conditions in order to produce the ultimate result; just as the randomness of the throw was combined with the equality of the two sides of the penny. This is quite sufficient to prevent us from obtaining anything which could strictly be called the objective probability of the events. So far as can be seen this example probably offers no exception to the general rule of a slowly but irregularly varying type. Even here therefore where the conceptions involved in the *à priori* theory seem most appropriate, they will be found, I think, to fail on examination.

§ 10. The reader who is familiar with Probability is of course acquainted with the celebrated theorem of Bernoulli. This theorem, of which the examples just adduced are merely particular cases, is generally expressed somewhat as follows:—that in the long run all events will tend to occur with a frequency proportional to their objective probabilities. With the ma-

* Physiologists, I believe, are of opinion that the relative ages of the father and mother have something to do with the sex of the offspring. If this be so, it completely bears out what is said above, for it introduces into the consideration an element dependent in some degree upon the civilization and sentiments of any particular age, an element which may possess any degree of irregularity. As a matter of fact, moreover, the proportion of 106 to 100 given above, does not seem by any means universal in all countries or at all times.

thematical proof of this theorem I have nothing to do here; nor, if there is any value in the foregoing criticism, need we trouble ourselves about it, for in that case the basis on which the mathematics rest is faulty, owing to the fact of there really being nothing which we can call the objective probability.

This theorem of Bernoulli seems to me one of the last remaining relics of Realism, which after being banished elsewhere still manages to linger in the remote province of Probability. It is an illustration of the inveterate tendency to objectify our conceptions even in cases where the conceptions had no right to exist at all. A uniformity is observed; sometimes, as in games of chance, it is found to be so connected with the physical constitution of the bodies employed as to be capable of being inferred beforehand, though even here the connection is by no means so necessary as is commonly supposed; this constitution is then converted into an "objective probability," supposed to develop somehow into the sequence which exhibits the uniformity. Finally, this very questionable objective probability is assumed to exist, with the same faculty of development, in *all* the cases in which uniformity is observed, however little resemblance there may be between these and games of chance.

§ 11. How utterly inappropriate any such conception is to most of the cases in which we find statistical uniformity, will be obvious on a moment's consi-

deration. The observed phenomena are generally the product, in these cases, of very numerous and complicated antecedents. The number of crimes, e. g. annually committed, is a function of the morality of the people, their social condition, and the vigilance of the police, each of these elements being in itself almost infinitely complex. Now as a result of all these agencies, there is some degree of uniformity, but what I have called above the *change of type* in it is most marked. The annual numbers fluctuate in a way which, however it may depend upon causes, shows none of the permanent uniformity of games of chance. This, combined with the obvious arbitrariness of singling out some only from the many antecedents which produced the regularity, would have been quite sufficient to prevent any one from assuming the existence of any objective probability here unless he had been predisposed to believe in it.

§ 12. I have been thus minute in the criticism of the above doctrine, because I think that, besides its intrinsic error, it has a strong tendency to obscure the due perception of two very important positive truths already mentioned; firstly, the gradual change of type, and secondly, the distinction between the actual series about which we reason and the substituted series we employ in reasoning about it.

Its bearing upon the first is obvious. The doctrine of an objective probability almost necessarily presup-

poses a fixed type. It seems merely the realistic doctrine of an ideal something which is perpetually striving, and gradually, though never perfectly, succeeding in realising itself in nature. Against this the view of a changeable type is, of course, distinctly antagonistic; still more so when, as I maintain, the type not merely changes, but has in many cases an actual origin and conclusion. I cannot help thinking also that the very common practice of carrying on statistical calculations almost indefinitely springs from, and is tainted by, the same error. If the type were fixed we could not have too many statistics, but if it vary, our extra labour may be worse than wasted. The danger of stopping too soon is easily seen, but in avoiding it we must not fall into that of going on too long.

The other caution is equally important. I have called attention to it already in the first chapter, and shall have to recur to it again at such length that I only state it here. The student cannot have too earnestly impressed upon him the fact that there is such a distinction.

§ 13. A possible objection may be raised here against calling the above results purely experimental, on the ground that it is the very essence of an average to diminish differences; that the arithmetical process of obtaining it insures this, whatever may be the properties of the things themselves. For instance, let

there be a party of ten men, of whom one or two are tall and one or two short, and take the average height of any five of them. Since this number cannot be made up of tall men only, or short men only, it stands to reason that the averages cannot differ so much amongst themselves as the single measures may. Is not then the equalizing process, it may be asked, which is observable on increasing the range of our observations, one which can be shown to follow from necessary rules of arithmetic, and one therefore which might be asserted *à priori?* Whatever apparent force there may be in this argument arises principally from the arbitrary limitations of the particular example given above, in which the number chosen was so large a proportion of the total as to exclude the possibility of only extreme cases being contained within it. Let us take an instance in which the number selected is by comparison small; suppose a succession of deaths, and examine the average duration of any ten lives. No possible reason can be assigned why any ten infants should not all live to the age of 100, or all die on the day after they were born. We can simply state the fact, discovered and proved by experience, that such averages are more nearly equal than the individual lives which compose them. It is doubtless quite true that if we take a given limited number of examples and divide them up into groups, we shall find that the averages of these groups present far

more of uniformity than the single examples of which these averages are composed. If the groups bear any considerable proportion to the whole this is necessarily the case. But it is quite a different thing to assert this when the groups do not bear more than a small proportion to the series out of which they are taken, still more so when the numbers in the series are practically or even really unlimited. The average height of the different batches of fifty men that might be selected from a party of eighty *must* be tolerably nearly equal; but there is no such obligation when the fifty are selected from one thousand, still less when they are selected from twenty millions. This latter is the characteristic of things as they present themselves in nature, and it is this which I assert to be solely established by experience.

The distinction may be made plainer by the help of another example. Suppose that we are considering a succession of throws of a penny. We select a limited number, say ten, and we assume it known that there are heads and tails amongst them. Both these restrictions are requisite before we can infer anything necessarily, in other words *à priori;* but let us see what is the real extent of this necessary inference. It seems simply this, that whereas when we select but a few of the ten we cannot tell but what they may be all heads or all tails, we may know for certain that by taking a large enough proportion

of the whole we escape such an extreme inequality, and must find specimens of both head and tail. But this is an extremely different thing from being able to assert that as we continue to take more and more out of the indefinite series of possible throws, we shall find the relative proportions of heads and tails gradually approaching towards equality. This indeed we are also able to assert, but from very different reasons. It can only be done by inductive extension from actual experience.

§ 14. The distinction which has just been described and illustrated corresponds in part to one which M. Quetelet has drawn between what he calls arithmetical averages and means, and to which he attaches extreme importance. It certainly is a very important distinction; but as his description of it, though interesting and very serviceable for practical purposes, seems to me to be involved in serious confusion in regard to the theory of the subject, I will quote and examine it at some length. It will serve as a good introduction to a fuller investigation of the nature of the series with which we are concerned in Probability than could be attempted in the last chapter.

He says*, "In measuring the height of a building twenty times in succession, I may not perhaps twice find the same identical value. However, it may be

* Quetelet *On Probabilities*, by O. G. Downes, p. 42.

conceived that the building has a determinate height; and if I have not exactly estimated it, in any one of the operations I have made to discover it, it is because these operations are liable to some uncertainty. I content myself, then, by taking the average of all my results as the true height sought. The limits, greater or smaller, depend on my skill or want of skill, and on the exactness of the instruments which I have used. I may employ the calculation of the mean in another sense. I wish to give an idea of the height of the houses in a certain street. The height of each of them must be taken, and the sum of the observed heights must be divided by the number of houses. The mean will not represent the height of any particular one; but it will assist in showing their height in general; and the limits, greater or smaller, will depend on the diversity of houses. There is between these two examples a very remarkable difference, which perhaps may not have been seen at first glance, but which is nevertheless of great importance. In the first, the mean represents a thing really in existence; in the second, it gives, in the form of an abstract number, a general idea of many things essentially different, although homogeneous. In another view (and this point is important) the numbers which have contributed to form the mean in the two examples present themselves in very different manners. In the second example, they are bound to one another by

no law of continuity; while in the first, as we shall soon have occasion to see, the determinations of the heights, although faulty, range themselves on each side of the mean with so great a regularity that their values might be predicted, if the limits within which they are comprised were given."

§ 15. The first remark I have to make upon this is to point out how the realistic doctrine, already alluded to, begins to creep in here. In one of the examples chosen there is a real and fixed value, viz. the height of the building, towards which the measurements strive to attain. But the same assumption is soon extended to cases in which there is no such fixed value. Thus for the measurements of the height of a house, are afterwards introduced those of the height of a man. This is all very well; but when, on comparing the group which these attempted measures compose, we find that they correspond roughly to the actual heights of a large number of *different* men taken at random, and go on to assume, as M. Quetelet does, that these actual heights must be in some way modelled on a type common to all: what is this but the reappearance of the realistic doctrine?

It might be true, as a matter of fact, that the two series of heights, viz. those of real men and those obtained by imperfect calculations from one model should be about the same; though, as Sir John Herschel has pointed out, considerable violence has in

reality to be done to the real heights before they can be forced into reasonable compliance with such a supposition. But even then there would be no meaning in asserting the real existence of the type or mean.

It is almost needless to point out that nothing which is said here is opposed to the conception of a type as employed by Comparative Anatomists. With them it is nothing more, I apprehend, than a statement of resemblances which are actually found to subsist between different species. If any additional hypothesis be intended, I presume it would be one of a causal nature as to the process by which these species and their varieties had been produced. This is quite different from the existence of a real type in the sense which M. Quetelet seems to contemplate.

§ 16. But there is a far more serious error, or, perhaps one should say, a far more serious confusion involved in the passage which has been quoted. A very important experimental result is slurred over in it; the different forms which this result may assume being not only treated as if they were necessarily alike in all examples, but the assumption being made that this form might have been ascertained deductively.

For clearness of conception let us first separate off the arithmetical average. This, as I have shown already, is entirely an abstraction of our own, and refers to a limited number of things or observations. If it embrace a large proportion of the whole number,

we may know for certain, except under special circumstances, that different averages so taken cannot differ from one another so much as the different single things may.

But now leaving this limited number, let us go on to contemplate the unlimited series from which the selection embraced by the average might be conceived to be taken. I may remind the reader that it does not matter, for our present purpose, of what sort of things this series is supposed to consist. The individuals which compose it may be presented to us in nature, or they may be products of our own in the form of attempted and erroneous measurements. In the latter case there is of course a real fixed value to which the measurements strive to attain; in the former case also it is often assumed that there must be something existing which occupies a similar position to the real and fixed value. I think this assumption is unwarranted, but in any case it is an inference with which we are not at present concerned. It is the series itself only which we are going to examine.

§ 17. When we investigate the different series which M. Quetelet has given as illustrative of what he terms a mean, it is true that we may detect in them a characteristic which was not brought under notice in our description of the distinctive series of Probability in the last chapter. But it is a characteristic, I apprehend, which does not in the slightest degree affect

the principles of the subject. It seems to me indeed to be one, which though inseparably connected with questions of Probability, is strictly speaking disconnected from the theory of the science.

The difference in fact between such examples as he mentions, and those which have hitherto occupied our attention, consists in the fact of the latter having what we have called a single type, and the former being composed of what might be called a group of types combined in a certain way. If, for instance, we examine a number of successive deaths we find about as many of them are male as female. But we may discover a great many more uniformities in them than this. Let us take a batch of ten. This batch may be made up in a variety of ways, according to the proportion of male and female deaths which compose it. If we repeat the process often enough we shall find that in a certain number of cases, and these the most numerous, the ten will be made up of five of each. In a somewhat smaller number it will be made up of six men and four women, in a still smaller number of seven men and three women, and so on. Similar proportions will hold if we substitute women for men. Here we have the notion of a mean, and all other examples of means will be found to resemble this in their essential characteristics. On analysis it is seen to be decomposable into a group of uniformities, each separate uniformity possessing the features characteristic of our science,

and the members of the group being united together by some definite law. This law of combination of the groups must be determined by experience, just like any of the features in the separate groups themselves. Of this law of combination I shall say no more at present, as I do not consider that it admits of any very accurate determination, and in any case it does not affect the general theory of our subject, which depends upon each separate uniformity preserving the essential feature belonging to examples in Probability.

§ 18. We now see clearly the distinction between the average and the mean. The former is simply the arithmetical average obtained in the ordinary way, and therefore referring necessarily to a limited number of things; it is moreover a pure abstraction of our own. If however these things are supposed to be a portion of an indefinite series of the well-known kind, they become fitting subjects for our science. If moreover several such series, whether composed of different sets of things or of the same things differently arranged, group themselves into symmetry, then we have the notion of a *mean*. As a matter of fact experience proves that almost all natural objects, when closely examined, are found to arrange themselves in groups of uniformities, and that there is throughout nature a tolerably regular law in accordance with which these groups are composed. Suppose a large number of persons born at the same time. If we examine the length of their

lives we shall find that they do not die, as it would be said, at random. A certain proportion will die in their first year, a certain proportion again in their second year, and so on. But these different proportions will themselves be found to be united together in a certain tolerably uniform manner. Again, if we examine a succession of deaths of men and women, we shall find, not merely that there are, on the average, about as many deaths of one sex as of the other, as many repetitions of two male deaths as of two female, and so on: we shall find that the numbers of these different repetitions are themselves grouped together in an orderly way, which becomes most marked when we take a very large number of examples. Take another example, of an entirely different kind. Suppose that a man had been practising for some time with a pistol at a target. The shots will be found to cluster about the centre, becoming less frequent the greater their distance from the centre. There will not only be about as many on one side as on the other at each different distance from the centre, but these numbers will be found to be graduated in an orderly and regular way in the long run.

Now in all these cases, any number more of which might be adduced, we observe the same characteristic. Not only do they present a collection of uniformities of the kind described so fully in our last chapter, but this collection is grouped in an orderly and regular

way. Hence if we have any means of finding out what is the law of connection of these groups, and can feel sure that there is but one law, we should then be able, from a fragment of one of these uniformities, to infer the whole of it, without a fresh appeal to experience.

In the foregoing remarks I have striven to draw out as clearly as I can what I conceive M. Quetelet to have been aiming at. But his description seems to me so confused that I may have failed to understand him. Be this as it may, the account given above appears to me to be a correct description of the facts as they are presented in nature.

§ 19. Let us now see what conclusions are drawn from the facts. These conclusions appear to be resolvable into a generalization, and an inference grounded upon that generalization. The generalization is that there is, if not one type for all the groups described above, at least one general principle upon which these types are founded. The inference is that since in certain cases there is undoubtedly a real objective thing at which the elements which go to make up the group are aimed, there must be something corresponding to this in all the cases. Of this inference I have already said all that seemed requisite; let us therefore turn for a moment to examine the generalization.

It is strange that this should ever have found acceptance, except as a rough approximation. Even M. Quetelet's own example, that of the heights of man-

kind, can only be brought into apparent agreement with his formula by the violent expedient of rejecting the extremes at both ends of the list on the plea that they are 'monstrosities.' This rejection is perfectly arbitrary, for these dwarfs and giants are born into the world like their better-shaped brethren and have precisely the same right to find themselves included in the formula. And even, by the help of this expedient, as has been mentioned already, the examples fit in but very lamely.

But there does not seem to be any need to appeal to particular examples, for there is the following obvious and notorious failure in the generalization. In some of the series which it attempts to embrace, e. g. successions of heads and tails in the throws of a penny, there is no finite limit to the individual fluctuations; in other cases, including almost all the applications of Probability to natural phenomena, there are such limits. This of course is well known, the reply generally being that these extreme deviations beyond a certain point are so excessively rare that no practical error is produced by assuming them to exist where in reality they do not. No practical error perhaps, but still enough to vitiate the theory, for the extreme cases are in every instance produced by the same agencies as those which produce all the intermediate ones; we cannot therefore conceive any alteration in the formula when it produced the one without in-

juring its integrity for the remainder. We cannot introduce hypothetical extreme variations, however scarce we make them, without making a consequent change, slight though it may be, in all the intermediate ones.

§ 20. If it be urged that the employment of one general rule (that familiar to mathematicians under the name of Least Squares) under very various circumstances, as for example in astronomical observations, &c. proves the validity of the generalization in question, I should reply that few persons have reflected upon the immense extent of experience that would be requisite for the purpose. To show that one rule gives correct results in the long run is not sufficient; it is necessary to show that no other rule would do as much. A very great number of observations would probably be necessary to show that of two formulæ, both of which recognized the principle that wide deviations from the mark were less common than small ones, only the one which admits a particular law for the diminution of these deviations was capable of being used with correct results. Only by such an investigation, I think, could it be ascertained that all the series which occur in nature do approximately conform to a common type, so as to be capable of being generalized under a common formula. They certainly do not accurately conform, for the reason given already, viz. that some of them admit infinite fluctuations whilst the majority do not.

§ 21. If the reader will carefully study the following example, one well known to mathematicians as the Petersburg Problem, I think it will serve to illustrate the three following considerations, at least, out of those which have occupied our attention in this and the preceding chapter :—(1) The distinction between the actual series of observation and the substituted one of calculation. (2) The fact that this latter is not hampered by the limits which experience imposes upon the former, and is therefore indefinite in the extent of its potential range. (3) That certain series take advantage of this indefinite range to keep on producing individuals in it whose deviation from the average has no finite limits whatever. When rightly viewed it is a very simple problem, but it has given rise to a great deal of confusion and perplexity.

The case is this :—a penny is tossed up ; if it gives head I receive two shillings ; if heads twice running four shillings ; if heads three times running eight shillings, and so on ; the amount doubling every time. In a word, however many times head may be given in succession, the number of shillings I may claim is found by multiplying two by itself that number of times. Here then is a series formed by a succession of throws. I will assume, — what most persons will consider to admit of demonstration, and what certainly experience confirms within consider-

able limits,—that the rarity of these 'runs' of the same face is in direct proportion to the amount I receive for them when they do occur. In other words, if we regard the occasions on which I receive payments, I shall find that every other time I get two shillings, once in four times I get four shillings, once in eight times eight shillings, and so on without end. The question is then asked, what ought I to pay for this privilege? At the risk of a slight anticipation of the next chapter, I may assume that this is equivalent to asking, what amount paid each time would, on the average, leave me neither winner nor loser? in other words, what is the average amount that I should receive on the above terms? Theory pronounces that I ought to give an *infinite* sum, that no finite sum, however great, would be an adequate equivalent. And this seems quite intelligible; there is a series of indefinite length before me, and the longer I continue to work it the richer are my returns, and this without any limit whatever. It is true that the very rich hauls are extremely rare, but still they do come, and, when they come, they make it up by their greater richness. On every occasion on which people have devoted themselves to the pursuit in question, they made acquaintance, of course, with but a limited portion of this series; but the series on which we base our calculation (which I have above described as the substituted or ideal

series) is unlimited, and the inferences we have drawn are in perfect accordance with this.

The common form of objection to this is given in the reply, that so far from paying an infinite sum no sensible man would give £50 for such a chance. Probably not, because no man would see enough of the series to make it worth his while. What most persons form their practical opinion upon, is such small portions of the series as they have actually seen or can reasonably expect. Now in any such portion, say of 100 throws, the longest succession of heads would not amount on the average to more than six or seven. This is observed, but it is forgotten that the formula which produced these, would, if it had greater scope, keep on producing better ones. Hence it arises that some persons are perplexed, because the conduct they would adopt in reference to the curtailed portion of the series which they practically meet with does not find its justification in inferences which are avowedly based on the series in the completeness of its infinitude. I shall have occasion to refer to the problem again in a future chapter.

§ 22. The results obtained in this chapter may be summed up as follows:—We have extended the conception of a series obtained in the last chapter; for we have found that nature presents these series to us in groups. These groups are divisible into two classes, which offer a marked contrast in the extreme cases

which they embrace, but offer a general similarity in other respects; though it would be hard to prove the existence of anything more between them than this general similarity. This similarity has given occasion, firstly to a sort of realistic inference, which seems almost unmeaning; and secondly to the belief that some of the series can be obtained deductively, which I have attempted to disprove. All that we can safely say about obtaining them is this, that by Inductive extension we may often, from a fragment of one of these series, infer the remainder of the series, and thence infer the other series which go to make up the group. A full discussion of the connection between Induction and Probability is reserved for a future chapter.

CHAPTER III.

GRADATIONS OF BELIEF.

§ 1. HAVING now obtained a clear conception of a certain kind of series, the next enquiry is, What is to be done with this series? How is it to be employed as a means of making inferences? The first step that we are now about to take might be described as one from the objective to the subjective, from the things themselves to the state of our minds in contemplating them.

The reader should observe that a substitution has already been made as a first stage towards bringing the things into a shape fit for calculation. This substitution, as described in the last chapter, is, in a measure, a process of *idealization*, corresponding, in our illustration, to the substitution of an ideal plane for the real bottom of the valley. The series we actually meet with show a changeable type, and the individuals of them will sometimes transgress their licensed irregularity. Hence they have to be pruned a little into shape, as natural objects always have before they are capable of being accurately reasoned about. The form in which the series emerges is that of a series with a

fixed type, and with its unwarranted irregularities omitted. This imaginary or ideal series is the basis of our calculation.

It must not be supposed that this is at all at variance with the assertion previously made, that Probability is a science of inference about real things; it is only by a substitution of the above kind that we are enabled to reason about the things. In nature they present themselves in a form not rigorously scientific, just as the bottom of the valley did; and as we had there to interpose an imaginary plane, so we have here to introduce an imaginary series. The only condition to be fulfilled in either case is, that the substitution is to be as little arbitrary, that is, to vary from the truth as slightly as possible. This kind of substitution generally passes without notice when natural objects of any kind are made subjects of exact science. I direct distinct attention to it here simply from the apprehension that want of familiarity with the subject-matter might lead some readers to suppose that it is, in this case, an exceptional deflection from accuracy in the formal process of inference.

I may remark also that this imaginary series offers no countenance whatever to the 'objective probability' doctrine criticised in the last chapter. It differs from anything contemplated on that hypothesis by the fact of its being recognized as a necessary substitution of

our own for the actual series, and to be kept in as close conformity with it as possible. It is a mere fiction or artifice necessarily resorted to for the purpose of calculation, and for this purpose only.

This caution is the more necessary, because in the example that I shall select, and which is one of the most favourite class of examples in this subject, the substitution becomes accidentally unnecessary. The things may sometimes need no trimming, because in the form in which they actually present themselves they *are* almost idealized. It is as if, at the bottom of our valley, we came upon a large sheet of ice; we could scarcely even imagine a more perfect plane than this. In most cases a good deal of alteration is necessary to bring the series into shape, but in some—I refer of course to games of chance—we find the alterations, for all practical purposes, needless.

§ 2. We start then, from such a series as this, upon the enquiry, What kind of inferences can be made about it? It may assist the logical reader to inform him that our first step will be analogous to one class of what are known as *immediate* inferences,—inferences, that is, of the type,—All men are mortal, therefore any particular man or men are mortal. This case, simple and obvious as it is in Logic, requires very careful consideration in Probability.

It is obvious that we must be prepared to form

an opinion upon the propriety of taking the step involved in such an inference. Hitherto we have had as little to do as possible with the irregular individuals; we have regarded them simply as fragments of a regular series. But we cannot long continue to neglect all consideration of them. Even if these events in the gross be certain, it is not only in the gross that we have to deal with them; they constantly come before us a few at a time or even as individuals, and we have to form some opinion about them in this state. An Insurance Office, for instance, deals with numbers large enough to obviate uncertainty, but each of their transactions has another party interested in it—What has the man who insures to say to their proceedings? for to him this question becomes an individual one. And even the Office itself receives its cases singly, and would therefore like to have as clear views as possible about these single cases. Now, the remarks made in the last two chapters about the subjects which Probability discusses might seem to preclude all enquiries of this kind, for was not ignorance of the individual presupposed to such an extent that even (as will be seen hereafter) causation might be denied without affecting our conclusions? The answer to this enquiry will require us to turn now to the consideration of a totally distinct side of the question, and one which has not yet come before us. Our

best introduction to it will be by the discussion of a special example.

§ 3. Let a penny be tossed up a very great many times; we may then be supposed to know for certain this fact (amongst many others) that in the long run head and tail will occur equally often. But suppose we consider only a moderate number of throws, or fewer still, and so continue limiting the number until we come down to three or two, or even one? We have as the extreme cases certainty or something undistinguishably near it, and utter uncertainty. Have we not, between these extremes, all gradations of belief? There is a large body of writers, including some of the most eminent authorities upon this subject, who reply that we are distinctly conscious of such a variation of the amount of our belief, and that this state of our minds can be measured and determined with almost the same accuracy as the external events to which they refer. The principal mathematical supporter of this view is Professor De Morgan, who has insisted strongly upon it in all his works on the subject. The clearest exposition of his opinions will be found in his Formal Logic, in which work he has made the view which we are now discussing the basis of his system. He holds that we have a certain amount of belief of every proposition which may be set before us, an amount which in its nature admits of determination, though we may

practically find it difficult in any particular case to determine it. He considers, in fact, that Probability is a sort of sister science to Formal Logic, speaking of it in the following words: "I cannot understand why the study of the effect, which partial belief of the premises produces with respect to the conclusion, should be separated from that of the consequences of supposing the former to be absolutely true." In other words, there is a science—Formal Logic—which investigates the rules according to which one proposition can be necessarily inferred from another; corresponding with this there is a science which investigates the rules according to which the amount of our belief of one proposition varies with the amount of our belief of other propositions with which it is connected.

§ 4. If this were the opinion of Professor De Morgan only, or even of mathematicians generally (and I believe that substantially the same opinion is adopted by all who have treated the subject mathematically), it might be objected that their peculiar studies had given them a bias towards discovering the distinctions and accuracy of numbers in matters into which these qualities are not commonly supposed to enter. But it must be observed that a professed logician, Archbishop Thomson, has to a considerable extent adopted the same opinion. In his work on the Laws of Thought he gives a distinct

section to the treatment of what he calls 'Syllogisms of Chance.' He prefaces it with a statement that the substance of the section is extracted from the works of Professor De Morgan, and others who agree with Professor De Morgan; he also makes a quotation from Professor Donkin, with which he seems to agree, which declares that the subject matter of the science of Probability is 'quantity of belief.' I must confess, with all respect to the Archbishop, that this chapter has always appeared to me less acute than the rest of his work. I refer to it here only in order to show that the opinion now under discussion is by no means confined to mathematicians, but has been recognized and adopted by men who certainly cannot be charged with being subject to a mathematical bias.

§ 5. Before proceeding to criticise this opinion I would make one remark upon it which has been constantly overlooked. It should be borne in mind that, even were this view of the subject not actually incorrect, it would nevertheless be insufficient for the purpose of a definition, inasmuch as variation of belief is not confined to Probability. It is a property with which that science is concerned, no doubt, but it is a property which meets us in many other directions as well. In every case in which we extend our inferences by Induction or Analogy, or depend upon the witness of others, or trust to our own me-

mory of the past, or come to a conclusion through conflicting arguments, or even make a long and complicated deduction by mathematics or logic, we have a result of which we can scarcely feel as certain as of the premises from which it was obtained. In all these cases then we are conscious of varying quantities of belief, but are the laws according to which the belief is produced and varied the same? If they cannot be reduced to one harmonious scheme, if in fact they can be brought to nothing but a number of different schemes each with its own body of laws and rules, then it is in vain to endeavour to force them into one science.

This opinion is strengthened by observing that most of the writers who adopt the definition in question do practically dismiss from consideration most of the above-mentioned examples of diminution of belief, and confine their attention to classes of events which have the property discussed in Chap. I. viz. 'ignorance of the few, knowledge of the many.' It is quite true that considerable violence has to be done to some of these examples, by introducing exceedingly arbitrary suppositions into them, before they can be forced to assume a suitable form. But still I have little doubt that, if we carefully examine the language employed, we shall find that in almost every case assumptions are made which virtually imply that our knowledge of the individual is derived from propositions given

in the typical form described in Chap. I. This will be more fully proved when we come to consider some common misapplications of the science.

§ 6. Even then, if the above-mentioned view of the subject were correct, it would yet be insufficient for the purpose of a definition; but it is at least very doubtful whether it is correct. Before we could properly assign to the belief side of the question the prominence given to it by Professor De Morgan and others, certainly before the science could be defined from that side, it would be necessary, I think, to establish the two following positions, against both of which strong objections can be brought.

(1) That our belief of every proposition is a thing which we can, strictly speaking, be said to measure. There must be a certain amount of it in every case, which we can realize somehow in consciousness and refer to some standard so as to pronounce upon its value.

(2) That the value thus apprehended is the correct one according to the theory, viz. that it is the exact fraction of full conviction that it should be. This statement will perhaps seem somewhat obscure at first; it will be explained presently.

§ 7. (1) Now, in the first place, as regards the difficulty of obtaining any measure of the amount of our belief. One source of this difficulty is too ob-

vious to have escaped notice; this is the disturbing influence produced on the quantity of belief by any strong emotion or passion. A deep interest in the matter at stake, whether it excite hope or fear, plays great havoc with the belief-meter, so that we must assume the mind to be quite unimpassioned in weighing the evidence. This is noticed and acknowledged by Laplace and others; but these writers seem to assume it to be the only source of error, and also to be of comparative unimportance. Even if it were the only source of error I cannot see that it would be unimportant. We experience hope or fear in so very many instances, that to omit such influences from consideration would be almost equivalent to saying that whilst we profess to consider the whole quantity of our belief we will in reality consider only a portion of it. Very strong feelings are, of course, exceptional, but we should nevertheless find that the emotional element, in some form or other, makes itself felt on almost every occasion. It is very seldom that we cannot speak of our surprise or expectation in reference to any particular event. Both of these expressions, but especially the former, seem to point to something more than mere belief. I know that the word 'expectation' is generally defined in treatises on Probability as equivalent to belief; but I doubt whether any one who attends to the popular use of the terms would admit that they were exactly syno-

nymous. Be this however as it may, the emotional element is present upon almost every occasion, and its disturbing influence therefore is constantly at work.

§ 8. Another cause, which co-operates with the former, is to be found in the extreme complexity and variety of the evidence on which our belief of any proposition depends. Hence it results that our belief is one of the most fugitive and variable things possible, so that we can scarcely ever get sufficiently clear hold of it to measure it. This is not confined to the times when our minds are in a turmoil of excitement through hope or fear. In our calmest moments we shall find it no easy thing to give a precise answer to the question, how firmly do I hold this or that belief? There may be one or two prominent arguments in its favour, and one or two corresponding objections against it, but this is far from comprising all the causes by which our state of belief is produced. Because such reasons as these are all that can be practically introduced into oral or written controversies, we must not conclude that it is by these only that our conviction is influenced. On the contrary, our conviction generally rests upon a sort of chaotic basis composed of an infinite number of inferences and analogies of every description, and these moreover distorted by our state of feeling at the time, dimmed by the degree of our recollection of them afterwards,

and probably received from time to time with varying force according to the way in which they happen to combine at the moment. To borrow a striking illustration from Abraham Tucker, the substructure of our convictions is not so much to be compared to the solid foundations of an ordinary building, as to the piles of the houses of Rotterdam which rest somehow in a deep bed of soft mud. They bear their weight securely enough, but it would not be easy to point out accurately the dependence of the different parts upon one another. Directly we begin to think of the amount of our belief, we have to think of the arguments by which it is produced—in fact, these arguments will intrude themselves without our choice. As each in turn flashes through the mind, it modifies the strength of our conviction; we are like a person listening to the confused hubbub of a crowd, where there is always something arbitrary in the particular sound we choose to listen to. There may be reasons enough to suffice abundantly for our ultimate choice, but on examination we shall find that they are by no means apprehended with the same force at different times. The belief produced by some strong argument may be very decisive at the moment, but it will often begin to diminish when the argument is not actually before the mind. It is like being dazzled by a strong light; the impression still remains, but begins almost immediately to fade away.

I think that this is the case, however we try to limit the sources of our conviction.

§ 9. (II) But supposing that it were possible to strike a sort of average of this fluctuating state, should we find this average to be of the amount assigned by theory? In other words, is our natural belief in the happening of two different events in direct proportion to the frequency with which those events happen in the long run? There is a lottery with 100 tickets and ten prizes; is a man's belief that he will get a prize fairly represented by one-tenth of certainty? The mere reference to a lottery should be sufficient to disprove this. Lotteries have flourished at all times, and have never failed to be abundantly supported, in spite of the most perfect conviction, on the part of many of those who put into them, that in the long run all will lose. Deductions should undoubtedly be made for those who act from superstitious motives, from belief in omens, dreams, &c. But apart from these, and supposing any one to come fortified by all that mathematics can do for him, I cannot believe that his natural impressions about single events would be always what they should be according to theory. Are there many who can honestly declare that they would have no desire to buy a single ticket? They would probably say to themselves that the sum they paid away was nothing worth mentioning to lose, and that there was a chance of gaining a great

deal; in other words, they are not apportioning their belief in the way that theory assigns.

What bears out this view is, that the same persons who would act in this way in single instances, would often not think of doing so in any but single instances. In other words, the natural tendency is to attribute too great an amount of belief where it is or should be small; i. e. to disparage the risk in proportion to the contingent advantage. They would very likely, when argued with, attach disparaging epithets to this state of feeling by calling it an unaccountable fascination, or something of that kind, but of its existence there can be little doubt. I am speaking now of what is the natural tendency of our minds, not of that into which they may at length be disciplined by education and thought. If, however, educated persons have succeeded for the most part in controlling this tendency in games of chance, the 'spirit of reckless speculation' has scarcely yet been banished from commerce. On examination, this tendency will be found, I think, so universal in all ages, ranks, and dispositions, that it would be inadmissible to neglect it in order to bring our supposed instincts more closely into accordance with the commonly received theories of Probability.

§ 10. There is another aspect of this question which has been often overlooked, but which seems to deserve some attention. Granted that we have an

instinct of credence, why should it be assumed that it must be just of that intensity which subsequent experience will justify? Our instincts are implanted in us by our Creator, and are intended to act immediately and unconsciously. They are, however, subject to control, and have to be brought into accordance with what we believe to be true and right. In other departments of psychology we do not assume that every spontaneous prompting of nature is to be left just as we find it, or even that on the average, omitting individual variations, it is set at that pitch that will be found in the end to be the best when we come to think about it and assign it its rules. Take, for example, the case of resentment. Here we have an instinctive tendency, and one that on the whole is good in its results. But moralists are agreed that almost all our efforts at self-control are to be directed towards subduing it and keeping it in its right direction. It is assumed to be given as a sort of rough protection, and to be set, if one might so express oneself, at too high a pitch to be deliberately and consciously acted on in society. May not something of this kind be the case also with our belief? I only make a passing reference to this point here, as on the theory of Probability adopted in this work it does not seem to be at all material to the science. But it seems a strong argument against the expediency of commencing the study of the science from the sub-

jective side, or even of assigning any great degree of prominence to this side.

That men *do* not believe in exact accordance with this theory must have struck almost every one, but this has probably been considered as mere exception and irregularity; the assumption being made that on the average, and in far the majority of cases, they do so believe. As stated above, I think it very doubtful whether the tendency which has just been discussed is not so universal that it might with far more propriety be called the law than the exception. And it may be better that it should be so: many good results may follow from that cheerful disposition which induces a man sometimes to go on trying after some great good, the chance of which he overvalues. He will keep on through trouble and disappointment, without serious harm perhaps, when the cool and calculating bystander sees plainly that his 'measure of belief' is much higher than it should be. So, too, the tendency also so common, of underrating the chance of a great evil may also work for good. To many men death might be looked upon as an almost infinite evil, at least they would so regard it themselves; suppose they kept this contingency constantly before them at its right value, how would it be possible to get through the practical work of life? Men would be stopping indoors because if they went out they might be murdered or bitten by a mad dog.

I am not advocating a return to our instincts; when we have once reached the critical and conscious state, it is not possible to do so; but it should be noticed that the advantage gained by correcting them is at best but a balanced one. What is most to our present purpose, it suggests the inexpediency of attempting to found an exact theory on what may afterwards prove to be a mere instinct, unauthorized in its full extent by experience.

§ 11. It may be replied, that though people, as a matter of fact, do not apportion belief in this exact way, yet they *ought* to do so. The purport of this remark will be examined presently; I will only say here that it grants all that I am contending for. For it admits that the degree of our belief is capable of modification, and may need it. But in accordance with what is the belief to be modified? obviously in accordance with experience; it cannot be trusted to by itself, but the fraction at which it is to be rated must be determined by the comparative frequency of the events to which it refers. Experience, then, furnishing the standard, it is surely most reasonable to start from this experience, and to found the theory of our process upon it.

If we do not do this it should be observed that we are detaching Probability altogether from the study of things external to us, and making it nothing else in effect than a portion of Psychology. If we

refuse to be controlled by experience, but confine our attention to the laws according to which belief is naturally or instinctively compounded and distributed in our minds, we have no right then to appeal to experience afterwards even for illustrations, unless under the express understanding that we do not guarantee its accuracy. Our belief in some single events, for example, might be correct, and yet that in a compound of several (if derived merely from our instinctive laws of belief) very possibly might not be correct, but might lead us to error if we determined to act upon it. Even if the two were in accordance, this accordance would have to be proved, which would lead us round, by what I cannot but think a circuitous process, to the point which has been already chosen for commencing with.

§ 12. Professor De Morgan seems to imply that the doctrine criticized above finds its justification from the analogy of Formal Logic. I confess I cannot see much force in the analogy. Formal Logic is based upon the assumption that there are laws of mind as distinguished from laws of things, and that these laws of mind can be ascertained and studied without taking into account their reference to any particular object. But to support this assumption a postulate has to be claimed or else a consequence faced. The postulate is, that the laws of the things are so far in harmony with those of our minds that

we may be certain that any exercise of our minds will not lead us into contradictions in practice. If this postulate be not granted we must then be prepared to brave any consequences that may follow from a want of such harmony. It is supposable, as some logicians seem ready to admit, that the laws of matter should be defiantly at variance with those of mind. So much the worse for us, but we cannot help it; we must go on thinking in accordance with our laws, for they are unhappily fixed for ever and invariable, and we must be content to take the consequences. But, as was briefly stated in § 11, no such distinction can be drawn in the case of laws of belief as we find them in Probability. Our instincts of credence are unquestionably in frequent hostility with experience; and what do we do then? We simply modify the instincts into accordance with the things. We are constantly performing this practice, and no cultivated mind would find it possible to do anything else. No man would think of divorcing his belief from the things on which it was exercised, or of thinking that the former had anything else to do than to follow the lead of the latter. Whatever then may be the claims of Formal Logic to rank as a separate science, it cannot, I think, furnish any support to the theory of Probability as conceived by some mathematicians.

§ 13. I have examined the doctrine in question

with a minuteness which may seem tedious, but in consequence of the eminence of its supporters it would have been presumptuous to have rejected it without the strongest grounds. The objections which have just been urged might be summarized as follows;—the amount of our belief of any given proposition, supposing it to be in its nature capable of determination (which is extremely doubtful), depends upon a great variety of causes, of which statistical frequency,—the subject of Probability—is but one. That even if we confine our attention to this one cause, the natural amount of our belief is not necessarily what theory would assign, but has to be checked by appeal to experience. The subjective side of Probability therefore, though very interesting and well deserving of examination, seems a mere appendage of the objective, and affords in itself no safe ground for a science of inference.

§ 14. The conception then of the science of Probability as a science of the laws of belief seems to me to break down at every point. We must not however rest content with such merely negative criticism. The degree of belief we entertain of a proposition may be hard to get at accurately, and when obtained may be often wrong, and need therefore to be checked by an appeal to the objects of belief. Still in popular estimation we do seem to be able with more or less accuracy to form a gra-

duated scale of intensity of belief. What we have to examine now is whether this be possible, and, if so, what is the explanation of the fact?

That it is generally believed that we can form such a scale scarcely admits of doubt. There is a whole vocabulary of common expressions such as, I feel almost sure; I do not feel quite certain; I am less confident of this than of that, &c. When we make use of any one of these phrases we never doubt that we have a distinct meaning to convey by means of it. Nor do we feel much at a loss, under any given circumstances, as to which of these expressions we should employ in preference to the others. If we were asked to arrange in order, according to the intensity of the belief with which we respectively hold them, things broadly marked off from one another, we could do it from our consciousness of belief alone, without a fresh appeal to the evidence upon which the belief depended. Passing over the looser propositions which are used in common conversation, let us take but one simple example from amongst those which furnish numerical data. Do I not feel more certain that some one will die this week in the town, than in the street in which I live? and if the town contain a population one hundred times greater than that in the street, would not almost any one assert unhesitatingly that he felt a hundred times more sure of the first proposition than of the second?

Here then a problem proposes itself. If popular opinion, as illustrated in common language, be correct,—and very considerable weight must of course be attributed to it,—there does exist something which we call partial belief in reference to any proposition of the numerical kind described above. Now what we want to do is to find some test or justification of this belief, to obtain in fact some intelligible answer to the question, Is it correct? We shall find incidentally that the answer to this question will throw a good deal of light upon another question, viz. what is the meaning of this partial belief?

§ 15. We shall find it advisable to commence by ascertaining how such enquiries as the above would be answered in the case of ordinary full belief. Such a step will not offer the slightest difficulty. Suppose, to take a simple example, that we have obtained the following proposition,—whether by Induction, or the rules of ordinary Logic, does not matter for our present purpose,—that a mixture of oxygen and hydrogen is explosive. Here we have an inference, and consequent belief of a proposition. Now suppose there were any enquiry as to whether our belief were correct, what should we do? The simplest way of settling the matter would be to find out by a distinct appeal to experience whether the proposition was true. Since we are reasoning about things, the justification of the belief, that is, the test of its correctness, would be

most readily found in the truth of the proposition. If by any process of inference I have come to believe that a certain mixture will explode, I consider my belief to be justified, that is to be correct, if under proper circumstances the explosion always occurs; if it does not occur the belief was wrong.

Such an answer, no doubt, goes but a little way, but it is sufficient for our present purpose. In all inferences about things, in which the *amount* of our belief is not taken into account, such an explanation as the above is quite sufficient; it would be the ordinary one in any question of science. It is moreover perfectly intelligible, whether the conclusion is particular or universal. Whether we believe that some men die, or that all men die, our belief may with equal ease be justified by the appropriate train of experience.

§ 16. But when we attempt to apply the same test to *partial* belief, we shall find ourselves reduced to an awkward perplexity. A difficulty now emerges which has been singularly overlooked by those who have treated of the subject. As a simple example will serve our purpose, we will take the case of a penny. I am about to toss one up, and I therefore half-believe, to adopt the current language, that it will give head. Now it seems to be overlooked that if we appeal to the event, as we did in the case last examined, our belief must inevitably be wrong, and

therefore the test above mentioned will fail. For the thing must either happen or not happen; i.e. in this case the penny must either give head, or not give it; there is no third alternative. But whichever way it occurs, our half-belief, so far as such a state of mind admits of interpretation, must be wrong. If head does come, I am wrong in not having expected it enough; for I only half believed in its occurrence. If it does not happen, I am equally wrong in having expected it too much; for I half believed in its occurrence, when in fact it did not occur at all.

The same difficulty will occur in every case in which we attempt to justify our state of partial belief in a single contingent event. Let us take another example, slightly differing from the last. A man is to receive £1 if a die gives six, to pay 1*s.* if it gives any other number. It will generally be admitted that he ought to give 2*s.* 6*d.* for the chance, and that if he does so he will be paying a fair sum. This example only differs from the last in the fact that instead of simple belief in a proposition, we have taken what mathematicians call 'the *value* of the expectation.' In other words, we have brought into greater prominence, not merely the belief, but the conduct which is founded upon the belief. But precisely the same difficulty recurs here. For appealing to the event,—the single event that is,—we see that one or other party must lose his money without

compensation. In what sense then can such an expectation be said to be a fair one?

§ 17. A possible answer to this, and so far as I can see the only possible answer, will be, that what we really mean by saying that we half believe in the occurrence of head is to express our conviction that head will certainly happen on the average every other time. And similarly, in the second example, by calling the sum a fair one it is meant that in the long run neither party will gain or lose. I am not sure that such an answer will be made; if it is I almost entirely agree with it. As we shall recur to it presently, the only notice that can be taken of it at this point is to call attention to the fact that it entirely abandons the whole question in dispute, for it admits that this partial belief does not in any strict sense apply to the individual event, for it clearly cannot be justified there. At such a result indeed we cannot be surprised, at least we cannot on the theory adopted throughout this Essay. For bearing in mind that the employment of Probability postulates ignorance of the single event, it is not easy to see how we are to justify any other opinion or statement about the single event than a confession of such ignorance.

§ 18. So far then we do not seem to have made the slightest approximation to a solution of the particular question now under examination. The more

closely we have analysed special examples, the more unmistakeably are we brought to the conclusion that in the individual instance no justification of anything like quantitative belief is to be found; at least none is to be found in the same sense in which we expect it in ordinary scientific conclusions, whether Inductive or Deductive. And yet we have to face and account for the fact that common impressions, as attested by a whole vocabulary of common phrases, are in favour of the existence of this quantitative belief. How are we to account for this? If we appeal to an example again, and analyze it somewhat more closely, we may yet find our way to some satisfactory explanation. I offer it however with some diffidence, for though it is the best to which, after much reflection, I can see my way, the enquiry is one which more properly belongs to those who have made a longer and more profound study of Psychology than I have been able to do.

In our previous analysis we found it sufficient to stop at an early stage, and to give as the justification of our belief the fact of the proposition being true. Stopping however at that stage we have found this explanation fail altogether to give a justification of partial belief; fail, that is, when applied to the individual instance. Suppose then we advance a step further in the analysis, and ask again what is meant by the proposition being true? This introduces us,

of course, to a very long and intricate path, but in the short distance along it which we shall advance, we shall not I hope find any very serious difficulty. As before, we will illustrate the analysis by first applying it to ordinary full belief.

§ 19. Whatever opinion then may be held about the essential nature of belief, it will probably be admitted that a readiness to act upon the proposition believed is an inseparable accompaniment of that state of mind. There can be no alteration in the belief without a possible alteration in the conduct, nor anything in the conduct which is not connected with something in the belief. We will first take an example in connection with the penny, in which, as I have said, there is full belief; we will analyse it a step further than we did before, and then attempt to apply the same analysis to an example of a similar kind, but one in which the belief is partial instead of full.

§ 20. Suppose that I am about to throw a penny up, and contemplate the prospect of its falling upon one of its sides and not upon its edge. Whatever else may be implied in our belief we certainly mean this; that we are ready to stake our conduct upon its falling thus. All our betting, and everything else that we do, is carried on upon this supposition. Any risk whatever that might ensue upon its falling otherwise will be incurred without fear. This, it must

be observed, is equally the case whether we are speaking of a single throw or of a long succession of throws.

But now let us take the case of a penny falling, not upon one side or the other, but upon a given side, *head*. To a certain extent this example resembles the last. We are perfectly ready to stake our conduct upon what comes to pass in the long run. When we are considering the result of a large number of throws, we are ready to act upon the supposition that head comes every other time. If, e. g. we are betting upon it, we shall not object to paying £1 every time that head comes, on condition of receiving £1 every time that head does not come. This is nothing else than the *translation*, as we may call it, into practice, of our belief that head and tail occur equally often.

Now it will be obvious, on a moment's consideration, that our conduct is capable of being slightly varied, of being varied, I mean, in form, whilst it remains identical in result. It is clear that to pay £1 every time we lose, and to get £1 every time we gain, comes to precisely the same thing, in the case under consideration, as to pay ten shillings every time without exception, and to receive £1 every time that head occurs. It is so, because heads occur, on the average, every other time. In the long run the two results coincide, but there is a marked difference between the two cases, considered individually. The difference is two-fold.

In the first place we have slid from the notion of a payment every other time, and come to that of one made every time. In the second place, what we pay every time is half of what we get in the cases in which we do get anything. The difference may seem slight; but mark the effect when our conduct is translated back again into the subjective condition upon which it depends, viz. into our belief. It is in consequence of such a translation, as it appears to me, that the notion has been acquired that we have an accurately determinable amount of belief as to every such proposition. To have losses and gains of equal amount, and to incur them equally often, was the experience connected with our belief that the two events, head and tail, would occur equally often. This was quite intelligible, for it referred to the long run. To find that this could be commuted for a payment made every time without exception, a payment, observe, of half the amount of what we occasionally receive, has very naturally been interpreted to mean that there must be a state of half-belief which refers to each individual throw.

§ 21. One such example, of course, does not go far towards establishing a theory. But the reader will bear in mind that almost all our conduct tends towards the same result,—that it is not in betting only, but in every circumstance in which we have to count the events, that such a numerical apportionment of

our conduct is possible. Hence, by the ordinary principles of association, it would appear exceedingly likely that, not exactly a numerical condition of mind, but rather, numerical associations become inseparably connected with each particular event. Once in six times a die gives ace; a knowledge of this fact, taken in combination with all the practical results to which it leads, produces, one cannot doubt, an inseparable notion of one-sixth connected with each *single* throw. But it surely cannot be called belief to the amount of one-sixth; at least it admits neither of justification nor explanation in these single cases, to which alone the fractional belief, if such existed, ought to apply.

It is in consequence, I apprehend, of such association that we act in such an unhesitating manner in reference to any single contingent event, even when we have no expectation of its being repeated. A die is going to be thrown up once, and once only. I bet 5 to 1 against ace, not, as is commonly asserted, because I feel one-sixth part of certainty in the occurrence of ace; but because I know that such conduct would be justified in the long run of such cases, and I apply to the solitary individual the same rule that I should apply to it if I knew it were one of a long series. This accounts for my conduct being the same in the two cases; by association, moreover, we probably experience very similar feelings in regard to them both.

§ 22. And here, on my view of the subject, we might stop. We are bound to explain the 'measure of our belief' in the occurrence of a single event when we judge solely from the statistical frequency with which such events occur, for such a series of events was our starting-point; but we are not bound to inquire whether in every case in which persons have, or claim to have, a certain measure of belief there must be such a series to which to refer it, and by which to justify it. Those who start from the subjective side, and regard Probability as the science of quantitative belief, are obliged to do this, but we are free from the obligation.

Still the question is one which is so naturally raised in connection with this subject, that it cannot be altogether passed by. I think that to a considerable extent such a justification as that mentioned above will be found applicable in other cases. The fact is that we are very seldom called upon to decide and act upon a single contingency which cannot be viewed as being one of a series. Experience introduces us, it must be remembered, not merely to a succession of events neatly arranged in a single series (as we have hitherto assumed them to be for the purpose of illustration), but to an infinite number belonging to a vast variety of different series. A man is obliged to be acting, and therefore exercising his belief about one thing or another almost the whole of

every day of his life. Any one person will have to decide in his time about a multitude of events, each one of which will never recur again within his own experience. But by the very fact of there being a multitude, though they are all of different kinds, we shall still find that order is maintained, and so a course of conduct can be justified. In a plantation of trees we should find that there is order of a certain kind if we measure them in any one direction, the trees being on an average about the same distance from each other. But a similar order would be found if we were to examine them in any other direction whatsoever. So in nature generally; there is regularity in a succession of events of the same kind. But there may also be regularity if we form a series by taking successively a number out of totally distinct kinds.

It is in this circumstance that we find an extension of the practical justification of the measure of our belief. A man, say, buys a life annuity, insures his life on a railway, puts into a lottery, and so on. Now we may make a series out of these acts of his, though each is in itself a single event which he never intends to repeat. His conduct, and therefore his belief, measured by the result in each individual instance, will not be justified, but the reverse, as shewn in § 16. Could he indeed repeat each kind of action often enough it would be justified, but from this, by the

conditions of life, he is debarred. Now it is perfectly conceivable that in the new series, formed by his successive acts of different kinds, there should be no regularity. As a matter of fact, however, it is found that there is regularity. In this way the equalization of his gains and losses for which he cannot hope in annuities, insurances, and lotteries separately, may yet be secured to him out of these events taken collectively. If in each case he values his chance at its right proportion (believing accordingly) he will in the course of his life neither gain nor lose. And in the same way if, whenever he has the alternative of different courses of conduct, he acts in accordance with the estimate of his belief described above, i.e. chooses the event whose chance is the best, he will in the end gain more in this way than by any other course. By the existence, therefore, of these *cross-series*, as we may term them, there is an immense addition to the number of actions which may be fairly considered to belong to those courses of conduct which offer many successive opportunities of equalizing gains and losses. All these cases then may be regarded as admitting of justification in the way now under discussion.

§ 23. In the above remarks it will be observed that we have been giving what is to be regarded as a justification of his belief from the point of view of the individual agent himself. If we suppose the existence of an enlarged fellow-feeling, the applicability of such

a justification becomes still more extensive. We can assign a very intelligible sense to the assertion that it is 999 to 1 that I shall not get a prize in a lottery, even if this be stated in the form that my belief in my so doing is represented by the fraction $\frac{1}{1000}$th of certainty. Properly it means that in a very large number of throws I should gain once in 1000 times. If we admit other contingencies of the same kind, as described in the last section, each individual may be supposed to reach to something like this experience within the limits of his own life. He could not do it in this particular line of conduct alone, but he could do it in this line combined with others. Now introduce the possibility of each man feeling that the gain of others offers some analogy to his own gains, which we may conceive his doing except in the case of the gains of those against whom he is directly competing, and the above justification becomes still more extensively applicable.

The following, I think, would be a fair illustration to test this view. I know that I must die on some day of the week, and there are but seven days. My belief, therefore, that I shall die on a Sunday is one-seventh. Here the contingent event is clearly one that does not admit of repetition; and yet would not the belief of every man have the value assigned it by the formula? I think that the same principle will be found to be at work here as in the former examples. It is

quite true that I have only the opportunity of dying once myself, but I am a member of a class in which deaths occur with frequency, and I form my opinion upon evidence drawn from that class. If, for example, I had insured my life for £1000, I should probably demand £7000 in case the office declared that it would only pay in the event of my dying on a Sunday. *I* might not find the arrangement an equitable one, but mankind at large, in case they acted on such a principle, might fairly commute their aggregate gains in such a way.

§ 24. The results of the last few sections might be summarized as follows:—the different amounts of belief which we entertain upon different events, and which are recognized by various phrases in common use, have undoubtedly some meaning. But their meaning, and certainly their justification, is to be sought in the *series* of corresponding events to which they belong; in regard to which it may be shown that far more events are capable of being referred to a series than might be supposed at first sight. The test and justification of belief are to be found in conduct; in this test applied to the series as a whole, there is nothing peculiar, it is like acting on our belief about any single thing. But so applied, it is applied successively to each of the individuals of the series; here our *conduct* generally admits of being in some way divided up numerically (I see no better way of

describing it) in reference to each particular event; and this has been understood to denote a certain amount of belief which should be a fraction of certainty. Probably on the principles of association, a peculiar condition of mind is produced in reference to each single event. And these associations are not unnaturally retained even when we contemplate any one of these single events isolated from any series to which it belongs. When it is found alone we treat it, and feel towards it, as we do when it is in company with the rest of the series.

§ 25. We may now see, more clearly than we could before, why it is that we are free from any necessity of assuming the existence of causation, in the sense of necessary invariable sequence, in the case of the events which compose our series. Against such a view it is sometimes urged, that we constantly talk of the probability of a single event; but how can this be done, it would probably be said, if we once admit the possibility of that event occurring fortuitously? Take an instance from human life; the average duration of the lives of any batch of men aged thirty will be about thirty-four years. We say therefore to any individual of them, Your expectation of life is twenty-five years. But how can this be said if we admit that his life is liable to be destitute of all regular sequence of cause and effect? I reply that the denial of causation enables us to

say neither more nor less than its assertion, in reference to the individual life, for of this we are ignorant in each case alike. By assigning, as above, an expectation in reference to the individual, we *mean* nothing more than to make a statement about the average of his class. Whether there be causation or not in these individual cases does not affect our knowledge of the average, for this by supposition rests on independent experience. The legitimate inferences are the same in each case, and of equal value. The only difference is that in the one case we have forced upon our attention the impropriety of talking of the 'proper' expectation of the individual, owing to the fact that all knowledge of its amount is formally impossible; in the other case the impropriety is overlooked from the fact of such knowledge being only practically unattainable. As a matter of fact the amount of our knowledge is the same in each case; it is a knowledge of the average, and of that only.*

§ 26. I conclude then that the limits within which we are thus able to justify the amount of our belief are far more extensive than might appear at first sight. Whether every case in which persons feel an amount of belief short of perfect confidence could be forced into the province of Probability is a wider question. Even, however, if the belief could be supposed capable of justification on its principles,

* For a fuller discussion of this, see Chap. XIV.

its rules could never in such cases be made use of. Suppose, for example, that a father were in doubt whether to give a certain medicine to his sick child. On the one hand the doctor declared the child would die unless the medicine were given; on the other, through a mistake, the father cannot feel quite sure that the medicine by him is the right one. It is conceivable that Laplace, in his conviction that everything is a probability, would declare that the man's belief had some 'value' (if he could only find out what it is), say nine-tenths; that this means that in nine cases out of ten in which he entertained a belief of that particular value it turned out that he was right. So with his doubt on the other side. Putting the two together there is but one course which, as a prudent man and a good father, he can possibly follow. It may be so, but when (as here) the identification of an event in a series depends on purely subjective conditions, as in this case upon the degree of vividness of his conviction, of which no one else can judge, no test is possible, and therefore no proof can be found. One dare not dogmatise by denying, and therefore, when any person gets the start by an assertion, he must be left in the field. A question very closely connected with this will be treated of in a future chapter on Common Misapplications of the Theory of Probability.

§ 27. So much then for the attempts, so fre-

quently made, to found the science on a subjective basis; they can lead, as I have endeavoured to show, to no satisfactory result. Still our belief is so inseparably connected with our action that something of a defence can be made for the attempts described above; but when it is attempted, as is often the case, to import other sentiments besides pure belief, and to find a justification for them also in the results of our science, the confusion becomes far worse. The following extract from Archbishop Thomson's *Laws of Thought* will show what kind of applications of the science are contemplated here: "In applying the doctrine of chances to that subject in connexion with which it was invented—games of chance,—the principles of what has been happily termed 'moral arithmetic' must not be forgotten. Not only would it be difficult for a gamester to find an antagonist on terms, as to fortune and needs, precisely equal, but also it is impossible that with such an equality the advantage of a considerable gain should balance the harm of a serious loss." "If two men," says Buffon, "were to determine to play for their whole property, what would be the effect of this agreement? The one would only double his fortune, and the other reduce his to naught. What proportion is there between the loss and the gain? The same that there is between all and nothing. The gain of the one is but a moderate sum,—

the loss of the other is numerically infinite, and morally so great that the labour of his whole life may not perhaps suffice to restore his property."

As moral advice this is all very true and good. But if it be regarded as a contribution to the science of the subject it is quite inappropriate, and seems calculated to cause confusion. The doctrine of chances pronounces upon certain kinds of events in respect of number and magnitude; it has absolutely nothing to do with any particular person's feelings about these relations. We might as well append a corollary to the rules of arithmetic, to point out that although it is very true that twice two are four it does not follow that four sheep will give twice as much pleasure to the owner as two will. If two men play on equal terms their chances are equal; in other words, if they were often to play in this manner each would lose as frequently as he would gain. That is all that Probability can say; what under the circumstances may be the determination and opinions of the men in question, it is for them and them alone to decide. There are many persons who cannot bear mediocrity of any kind, and to whom the prospect of doubling their fortune would outweigh a greater chance of losing it altogether. They alone are the judges.

If we will introduce such a balance of pleasure and pain the individual must make the calculation for

himself. The supposition is that total ruin is very painful, partial loss painful in a less proportion than that assigned by the ratio of the losses themselves; the inference is therefore drawn that on the average more pain is caused by occasional great losses than by frequent small ones, though the money value of the losses in the long run may be the same in each case. But if we suppose a country where the desire of spending largely is very strong and where from abundant production loss is easily replaced, the calculation might incline the other way. Under such circumstances it is quite possible that more happiness might result from playing for high than for low stakes. The fact is that all emotional considerations of this kind are irrelevant; they are, at most, mere applications of the theory, and such as each individual is alone competent to make for himself.

§ 28. It is by the introduction of such considerations as these that the really very simple and intelligible Petersburg Problem has been so perplexed. Having already given some description of this problem I will refer to it very briefly here. It presents us with a sequence of sets of throws for each of which sets I am to receive something. My receipts increase in proportion to the rarity of each particular kind of set, and each kind is found to grow more rare in a certain definite but unlimited order. By the wording of the problem, properly interpreted, I

am supposed never to stop. Clearly therefore, however large a fee I pay for each of these sets, I shall be sure to make it up in time. The mathematical expression of this is, that I ought always to pay an infinite sum. To this the objection is opposed, that no sensible man would think of advancing even a large finite sum, say £1000. Certainly he would not; but why? Because neither he nor those who are to pay him would be likely to live long enough for him to obtain throws good enough to remunerate him for one-tenth of his outlay; to say nothing of his trouble and loss of time. We must not suppose that the problem, as stated in the ideal form, will coincide with the practical form in which it presents itself in life. A carpenter might as well object to Euclid's second postulate, because his plane came to a stop in six feet on the plank on which he was at work. Many persons have failed to perceive this, and have assumed that, besides enabling us to draw numerical inferences about the members of a series, the theory ought also to be called upon to justify all the opinions which average respectable men might be inclined to form about them, as well as the conduct they might choose to pursue in consequence. It is obvious that to enter upon such considerations as these is to diverge from our proper ground. We are concerned, in these cases, with the actions of men only, as given in statistics; with the emotions they experience in the performance

of these actions we have no direct concern whatever. The error is the same as if any one were to confound, in political economy, value in use with value in exchange, and object to measuring the value of a loaf by its cost of production, because bread is worth more to a man when he is hungry than it is just after his dinner.

§ 29. One class of emotions indeed ought to be excepted, which, from the apparent uniformity and consistency with which they show themselves in different persons and at different times, do really present some better claim to consideration. In connection with a science of inference they can never indeed be regarded as more than an accident of what is essential to the subject, but compared with other emotions they seem to be inseparable accidents.

The reader will remember that attention was drawn in the earlier part of this chapter to the compound nature of the state of mind which we term belief. It is partly intellectual, partly also emotional; it professes to rest upon experience, but in reality the experience acts through the distorting media of hopes and fears and other disturbing agencies. So long as we confine our attention to the *state of mind* of the person who believes, it appears to me that these two parts of belief are quite inseparable. Indeed, to speak of them as two parts may convey a wrong impression; for though they spring from different sources, they so entirely merge in one result as to produce what might

be called a simple compound. Every kind of inference, whether in probability or not, is liable to be disturbed in this way. A timid man may honestly believe that he will be wounded in a coming battle, when others, with the same experience but calmer judgments, see that the chance is too small to deserve consideration. But such a man's belief, if we look only to that, will not differ from sound belief. His conduct also in consequence of his belief will by itself afford no ground of discrimination; he will make his will as sincerely as a man on his death-bed. The only resource is to check his belief by appealing to past and current experience. This was advanced as an objection to the theory on which probability is regarded as concerned primarily with laws of belief. But on the view taken in this Essay in which we are supposed to be concerned with laws of inference about things, error and difficulty from this source vanish. Let us bear clearly in mind that we are concerned with inferences about things, and are always to test our belief by experience of the things, and whatever there may be in belief which does not depend on experience will disappear from notice. It is conceivable indeed that men might be in such a state of abject panic that their senses, at the time or afterwards, were disturbed like their judgment beforehand. If so, we must appeal to the wider experience of other men of calmer mind, or to their own judgment in

their better moments. These are the ultimate, and apparently the only ultimate courts of appeal.

§ 30. The only notice then that these emotions can claim as an integral portion of any science of inference is, that they should be rigidly excluded from it. But if any of them are uniform and regular in their production and magnitude, they may be fairly admitted as accidental and extraneous accompaniments. This is really the case to some extent with our surprise. This emotion does show a considerable degree of uniformity. The rarer any event is the more am I, in common with most other men, surprised at it when it does happen. This surprise may range through all degrees, from the most languid form of interest up to the condition which we term 'being startled.' And since the surprise seems to be pretty much the same, under similar circumstances, at different times, and in the case of different persons, it is free from that extreme irregularity which is found in most of the other mental conditions which accompany the contemplation of unexpected events. Hence our surprise, though, as stated above, having no proper claim to admission into the science of Probability, is such a constant and regular accompaniment of that which Probability is concerned with, that notice must often be taken of it. References will occasionally be found to this aspect of the question in the following chapters.

It may be remarked in passing, for the sake of further illustration of the subject, that this emotional accompaniment of surprise, to which we are thus able to assign something like a fractional value, differs in two important respects from the commonly accepted fraction of belief. In the first place, it has what may be termed an independent existence; it is intelligible by itself. The belief, as I endeavoured to show, needs explanation and finds it in our consequent conduct. Not so with the emotion; this stands upon its own footing, and may be examined in and by itself. Hence, in the second place, it is as applicable, and as capable of any kind of justification, in relation to the *single event*, as to a series of events. In this respect, as will be remembered, it offers a complete contrast to our state of belief about any one contingent event. May not these considerations help to account for the general acceptance of the doctrine, that we have a certain definite and measurable amount of belief about these events? I cannot help thinking that what is so obviously true of the emotional portion of the belief, has been unconsciously transferred to the other or intellectual portion of the compound condition, to which it is not applicable, and where it cannot find a justification.

§ 31. A further illustration may now be given of the subjective view of Probability at present under discussion.

An appeal to common language is always of service, as the employment of any distinct word is generally a proof that mankind have observed some distinct properties in the things which have caused them to be singled out, and have that name appropriated to them. There is such a class of words assigned by popular usage to those events (amongst others) of which Probability takes account. If we examine them we shall find, I think, that they direct us unmistakeably to the two-fold aspect of the question,—the objective and the subjective, the quality in the events and the state of our minds in considering them,—that have occupied our attention during the former chapters.

The word 'extraordinary,' for instance, seems to point to the observed fact, that events are arranged in a sort of *ordo* or rank. No one of them might be so exactly placed that we could have inferred its position, but when we take a great many into account together, running our eye, as it were, along the line, we begin to see that they really do for the most part stand in order. Those which stand away from the line have this divergence observed, and are called extraordinary, the rest ordinary, or in the line. So too 'irregular' and 'abnormal' are doubtless used from the appearance of things, when examined in large numbers, being that of an arrangement by rule or measure. This only holds when there are a good

many; we could not speak of single events being so arranged. Again, the word 'law,' in its philosophical sense, has now become quite popularized. How the term became introduced is not certain, but I think there can be little doubt that it was somewhat in this way:—The observed effect of a law is to produce regularity where it did not previously exist; when then a regularity began to be perceived in nature, the same word was used, whether the cause was supposed to be the same or not. In each case there was the same generality of agreement, subject to occasional deflection*.

On the other hand, observe the words 'wonderful,' 'unexpected,' 'incredible.' Their connotation describes states of mind simply; they are not confined to Probability, but mean that the events they denote are such as from some cause we did not expect would happen, and at which therefore, when they do happen, we are surprised.

Now when we bear in mind that these two classes of words are in their origin perfectly distinct;—the one denoting simply events of a certain character; the other, though also denoting events, *connoting* simply states of mind;—and yet that they are universally applied to the same events, so as to be used as per-

* This would still hold of *empirical* laws which may be capable of being broken: we now have very much shifted the word, to denote an *ultimate* law which it is supposed cannot be broken.

fectly synonymous, we have in this a striking illustration of the two sides under which Probability may be viewed, and of the universal recognition of a close connection between them. The words are popularly used as synonymous, and we must not press their meaning too far; but if it were to be observed, as I think it could, that the application of the words which denote mental states is wider than that of the others, we should have an illustration of what has been already observed, viz. that the Province of Probability is not so extensive as that over which variation of belief might be observed. Probability only considers the cases in which this variation is brought about in a certain definite statistical way.

§ 32. It will be found in the end both interesting and important to have devoted some attention to this subjective side of the question. In the first place, as a mere speculative inquiry the quantity of our belief of any proposition deserves notice. To study it at all deeply would be to trespass into the province of Psychology, but it is so intimately connected with our own subject that we cannot avoid all reference to it. We therefore discuss the laws under which our expectation and surprise at isolated events increases or diminishes, so as to account for these states of mind in any individual instance, and, if necessary, to correct them when they vary from their proper amount.

But there is another more important reason than this. It is quite true that when the subjects of our discussion in any particular instance lie entirely within the province of Probability, they may be treated without any reference to our belief. We may or we may not employ this side of the question according to our pleasure. If, for example, I am asked whether it is more likely that A. B. will die this week, or that it will rain to-morrow, I may calculate the chance (which really is at bottom the same thing as my belief) of each, find them respectively, one-sixteenth and one-seventeenth, say, and therefore decide that my 'expectation' of the former is the greater, viz. that it is the more likely event. In this case the process is precisely the same whether we suppose our belief to be introduced or not; our mental state is, in fact, quite immaterial to the question. But, in other cases, it may be different. Suppose that we are comparing two things, of which one is wholly alien to Probability, the only ground they have in common may be the amount of belief to which they are respectively entitled. We cannot compare the frequency of their occurrence, for one may occur too seldom to judge by, perhaps it may be unique. It has been already said, that our belief of many events rests upon a very complicated and extensive basis. My belief may be the product of many conflicting arguments, and many analogies more or less remote;

these proofs themselves may have mostly faded from my mind, but they will leave their effect behind them in a weak or strong conviction. At the time, therefore, I may still be able to say, with some degree of accuracy, though a very slight degree, what amount of belief I entertain upon the subject. Now we cannot compare things that are heterogeneous; if, therefore, we are to decide between this and a thing determined by Probability, it is impossible to appeal to chances or frequency of occurrence. The measure of belief is the only common ground, and we must therefore compare this quantity in each case. The test afforded will be an exceedingly rough one, for the reasons mentioned above, but it will be better than none; in some cases it will be found to furnish all we want.

Suppose, for example, that one letter in a million is lost in the Post Office, and that in any given instance I wish to know, which is more likely, that a letter has been so lost, or that my servant has stolen it? If the latter alternative could, like the former, be stated in a numerical form, the comparison would be simple. But it cannot be reduced to this form, at least not consciously and directly. Still, if we could feel that our belief in the man's dishonesty was greater than one-millionth, we should then have homogeneous things before us, and therefore comparison would be possible.

§ 33. We are now in a position to give a toler-

ably accurate definition of a phrase which we have frequently been obliged to employ, or incidentally to suggest, and of which the reader may have looked for a definition already, viz. the probability of an event, or what is equivalent to this, the chance of any given event happening. I consider that these terms presuppose a series; within the indefinite class which composes this series a smaller class is distinguished by the presence or absence of some attribute or attributes, as was fully illustrated and explained in a previous chapter. These larger and smaller classes respectively are commonly spoken of as instances of the 'event,' and of 'its happening in a given particular way.' Adopting this phraseology, which with proper explanations is suitable enough, I should define the probability or chance (I regard the terms as synonymous) of the event happening in that particular way as the numerical fraction which represents the proportion between the two different classes in the long run. Thus, for example, let the probability be that of a given infant living to eighty. The larger series will comprise all men, the smaller all who live to eighty. Let the proportion of the former to the latter be 100 to 1; in other words, suppose that one child in a hundred lives to eighty. Then the chance or probability that any given child will live to eighty is the numerical fraction $\frac{1}{100}$. This assumes that

the series are of indefinite extent, and of the kind which we have described as possessing a fixed type. If this be not the case, but the series be supposed terminable, or irregularly fluctuating, then in so far as this is the case the series ceases to be a subject of science. What we have to do under these circumstances, is to substitute a series of the right kind for the inappropriate one presented by nature, choosing it, of course, with as little deflection as possible from the observed facts. This is nothing more than has to be done, and invariably is done, whenever natural objects are made subjects of strict science.

A word or two of explanation may be added about the expression employed above, 'the proportion in the long run.' The run must be supposed to be very long indeed, in fact never to stop. As we keep on taking more terms of the series we shall find the proportion still fluctuating a little, but its fluctuations will grow less. The proportion, in fact, will gradually approach towards some fixed numerical value, what mathematicians term its *limit*. This fractional value is the one spoken of above. In the few cases in which deductive reasoning is possible, this fraction may be obtained without direct appeal to statistics, from reasoning about the conditions under which the events occur, as was explained in the second chapter.

Here becomes apparent the full importance of the distinction so frequently insisted on, between the actual irregular series before us and the substituted one of calculation, and the meaning of the assertion (Ch. I. § 14), that it was in the case of the latter only that strict scientific inferences could be made. For how can we have a 'limit' in the case of those series which ultimately exhibit irregular fluctuations? When we say, for instance, that it is an even chance that an unvaccinated person recovers from the small-pox, the meaning of this assertion is that in the long run each alternate person attacked by that disease does recover. But if we examined a sufficiently extensive range of statistics, we might find that the manners and customs of society had produced such a change in the type of the disease or its treatment, that we were no nearer approaching towards a fixed limit than we were at first. The conception of an ultimate limit in the ratio between the numbers of the two classes in the series necessarily involves an absolute fixity of the type. When therefore nature does not present us with this absolute fixity, as she scarcely ever does except in games of chance (and not demonstrably there), our only resource is to invent such a series, in other words, as has so often been said, to substitute a series of the right kind.

The above, which I consider to be tolerably complete as a definition, might equally well have been

given in the last chapter. I have deferred it however to the present place, in order to connect with it at once a proposition involving the conceptions introduced in this chapter; viz. the state of our own minds, in reference to the amount of belief we entertain in contemplating any one of the events whose probability has just been described. Reasons were given against the opinion that our belief admitted of any exact apportionment like the numerical one just mentioned. Still, it was shown that a reasonable explanation could be given of such an expression as, my belief is $\frac{1}{10}$th of certainty, though it was an explanation which pointed unmistakeably to a series of events, and ceased to be intelligible unless viewed in such a relation to a series. In so far, then, as this explanation is adopted, we may say that our belief is in proportion to the above fraction. This referred to the purely intellectual part of belief which I cannot conceive to be separable, even in thought, from the things upon which it is exercised. With this intellectual part there are commonly associated many emotions. These we can to a certain extent separate, and, when separated, can measure with that degree of accuracy which is possible in the case of other emotions. They are moreover intelligible in reference to the individual events. They will be found, I think, to increase and diminish in accordance, to some extent, with the fraction which repre-

sents the scarcity of the event. The emotion of surprise does so with some degree of accuracy.

The above investigation describes, though in a very brief form, the amount of truth which appears to me to be contained in the assertion frequently made, that the fraction of probability represents also the fractional part of full certainty to which our belief of the individual event amounts. Any further analysis of the matter would seem to belong to Psychology rather than to Probability.

CHAPTER IV.

THE RULES OF INFERENCE IN PROBABILITY.

§ 1. In the previous chapter, an investigation was made into what may be called, from the analogy of Logic, Immediate Inferences. Given that nine men out of ten live to forty, what could be inferred about the prospect of life of any particular man? It was shown that, although this step was very far from being so simple as it is commonly supposed to be, and as the corresponding step really is in Logic, there was nevertheless an intelligible sense in which we might speak of the amount of our belief in any one of these proportional propositions, and justify that amount. We must now proceed to the consideration of inferences more properly so called, I mean inferences of the kind which form the staple of ordinary logical treatises. In other words, having ascertained in what manner particular propositions could be inferred from the general propositions which included them, we must now examine in what cases one general proposition can be inferred from another. By a general proposition here is meant, of course, a general proposition of the statistical kind contemplated in Probability.

The rules of such inference being very few and simple, their consideration will not detain us long.

§ 2. From the data now in our possession we are able to deduce the rules of probability given in ordinary treatises upon the science. It would be more correct to say that we are able to deduce *some* of these rules, for, as will appear on examination, they are of two very different kinds, resting on entirely distinct grounds. They might be divided into those which are formal, and those which are merely experimental. This may be otherwise expressed by saying that, from the kind of series described in the first two chapters, some rules will follow necessarily by the mere application of arithmetic; whilst others either depend upon peculiar hypotheses, or demand for their establishment continually renewed appeals to experience, and extension by the aid of Induction. We shall confine our attention at present principally to the former class; the latter can only be fully understood when we have considered the connection of our science with Induction.

§ 3. (1) We can make inferences by simple addition. If, for instance, there are two distinct properties observable in various members of the series, which properties do not occur in the same individual; it is plain that in any large batch, the number that are of one kind or the other will be equal to the sum of those of the two kinds separately. One man in ten,

say, is over six feet in height, and one in twelve is under five. Take a large number, say 120,000, then there will be about 12,000 tall and 10,000 short men amongst them; obviously therefore those who are of one kind or the other will be 22,000 in number. This rule, in its general algebraical form, would commonly be expressed in the language of Probability as follows:—If the chances of two incompatible events be respectively $\frac{1}{m}$ and $\frac{1}{n}$, the chance of one or other of them happening is $\frac{1}{m} + \frac{1}{n}$ or $\frac{m+n}{mn}$. Similarly if there were more than two such events. On the principles adopted in this Essay the rule, when thus expressed, means precisely the same thing as when it is expressed in the statistical form. It was shown, at the conclusion of the last chapter, that to say, for example, that the chance of a given event happening in a certain way is $\frac{1}{6}$, is only another way of saying that it does happen in that way once in six times.

It is plain that a corollary to this rule might be obtained, in precisely the same way, by subtraction instead of addition. Stated generally it would be as follows:—If the chance of one or other of two incompatible events be $\frac{1}{m}$, and the chance of one alone be $\frac{1}{n}$, the chance of the remaining one will be $\frac{1}{m} - \frac{1}{n}$ or $\frac{n-m}{mn}$.

Ex. If the chance of being either shot or bayoneted in a battle is $\frac{1}{2}$, and that of being shot is $\frac{2}{20}$, then that of being bayoneted is $\frac{1}{20}$. (Supposing that a man cannot be both shot and bayoneted).

§ 4. (2) We can also make inferences by multiplication. Suppose that the two events, instead of being incompatible as in the previous examples, are invariably connected together. Let a certain proportion of the members of the series possess a given property, and a certain proportion again of these, and of these only, possess another property, then the proportion which possess both properties is found by multiplying together the two fractions which represent the above two proportions. One man in ten, say, is over six feet in height, and one in fifty of these tall men, and of them only, has red hair; then, of the men whom we casually meet, about one in 500 will be tall and red-haired.

This rule is variously expressed in the language of Probability; perhaps the following is the commonest form:—If the chance of one event is $\frac{1}{m}$, and the chance that if it happens another will also happen is $\frac{1}{n}$, the consequent chance of the latter is $\frac{1}{mn}$.

The above inferences are necessary, in the sense in which arithmetical laws are supposed to be necessary, and they do not demand for their establishment any

arbitrary hypothesis. We assume in them no more than is warranted by the data actually given to us, and make our inferences from these data by the help of arithmetic. The formula, however, which we are about to examine next stands on a somewhat different footing.

§ 5. (3) In the two former rules we considered cases in which the data were supposed to be given under the conditions that the properties which distinguished the different kinds of events whose frequency we discussed, were respectively known to be disconnected and known to be connected. Let us now suppose that no such conditions are given to us. One man in ten, say, has red hair, and one in twelve stammers; what conclusions could we then draw as to the chance of any given man having one only of these two attributes, or neither, or both? It is clearly possible that the properties in question might be inconsistent with one another, so as never to be found combined in the same person; or all the stammerers might have red hair; or the properties might be allotted* in almost any proportion whatever. If we are perfectly

* I say, *almost* any proportion, because, as may easily be seen, arithmetic imposes certain restrictions upon the assumptions that can be made. We could not, for instance, suppose that all the red-haired men are stammerers, for in any given batch of men the former are more numerous. But the range of these restrictions is limited, and their existence is not of importance in the above discussion.

ignorant upon these points, it would seem that no inferences whatever could be drawn about the required chances.

Inferences however *are* drawn. An escape from the apparent indeterminateness of the problem, as above described, is found by assuming that, not merely will one-tenth of the whole number of men have red hair (for this was given as one of the data), but also that one-tenth alike of those who do and who do not stammer have red hair. Let us take a batch of 1200, as a sample of the whole. Now, from the data which were originally given to us, it will easily be seen that in every such batch there will be on the average 120 who have red hair, and therefore 1080 who have not. And here by rights we ought to stop, at least until we have appealed again to experience; but we do not stop here. From data which we have manufactured for ourselves we go on to infer that of the 120, 10 (*i. e.* one-twelfth of 120) will stammer, and 110 (the remainder) will not. Similarly we infer that of the 1080, 90 stammer, and 990 do not. On the whole, then, the 1200 are thus divided:—red-haired stammerers, 10; stammerers without red hair, 90; red-haired men who do not stammer, 110; men who neither stammer nor have red hair, 990.

This rule, expressed in its most general form in the language of Probability, would be as follows:—If the chances of a thing being p and q are respect-

ively $\frac{1}{m}$ and $\frac{1}{n}$, then the chance of its being both p and q is $\frac{1}{mn}$, p and not q is $\frac{n-1}{mn}$, q and not p is $\frac{m-1}{mn}$, not p and not q is $\frac{(m-1)(n-1)}{mn}$, where p and q are independent.

§ 6. The assumption in the last section is there given in its most glaring form. I cannot but think however that most writers on the subject do implicitly adopt it as it there stands, implying that where we know nothing about the distribution of the properties alluded to we must assume them to be distributed as above described, and therefore apportion our belief in the same ratio. This is called 'assuming the events to be independent,' the supposition being made that the rule will certainly follow from this independence, and that we have a right, if we know nothing to the contrary, to assume that the events are independent.

The validity of this last claim has already been discussed in the first chapter; it is only another of the attempts to construct *à priori* the series which experience will present to us, and one for which no such strong defence can be made as for the equality of heads and tails in the throws of a penny. But the meaning to be assigned to the 'independence' of the events in question demands a moment's consideration.

The circumstances of the problem are these. There

are two different qualities, by the presence or absence of which amongst the individuals of a series two distinct pairs of classes of these individuals are produced. For the establishment of the rule under discussion it was found that one supposition was both necessary and sufficient, namely, that the division caused by each of the above distinctions should subdivide each of the classes in the other pair in the same ratio in which it subdivides the whole. If the independence be granted and so defined as to mean this, the rule of course will stand, but, without especial attention being drawn to the point, it does not seem that the word would naturally be so understood.

§ 7. The above are the principal fundamental rules of inference which the science can give us. A few remarks may now be added about the form which they assume in some other works upon the subject. Reference has already been made to Professor De Morgan's assertion* about the province of Probability, that it has to study "the effect which partial belief of the premises produces with respect to the conclusion," whereas in ordinary logic we suppose the premises to be absolutely true. This will be the fittest place for explaining clearly my reasons for differing from him. Let us recur to the first of the examples quoted in this chapter; it was as follows:—One man in ten is over six feet high, one in twelve is under

* De Morgan's *Formal Logic*, Preface, p. v.

five; from this we inferred that eleven in sixty were not between five and six feet. These propositions, when stated in the form of a chance, would be expressed as follows. The chance of a man being over six feet is $\frac{1}{10}$, that of his being under five is $\frac{1}{12}$; therefore the chance of his not being between these heights is $\frac{11}{60}$. It has been stated, and fully explained, that these two forms of assertion mean precisely the same thing.

But it was also shown that there was a subjective side of the question, in accordance with which these propositions might assume the following form. My belief that a man will be over six feet is represented by $\frac{1}{10}$. There is no need to recur to this beyond reminding the reader, that a proposition of this kind only became intelligible or capable of justification when viewed in connection with the statistical facts to which it referred. Now Professor De Morgan seems to hold that these propositions, in the latter form, viz. in the form of statements of partial belief, can be inferred one from the other. To me it seems, on the contrary, that but little meaning and certainly no security can be attained by so regarding the process of inference. These probabilities must first be supposed to be re-translated into statements about the things, and then the inferences must be drawn from observations upon these things. This part of the operation is carried on, as already shown, by the

ordinary rules of arithmetic. The conclusion, when obtained, may, of course, be stated in the subjective form, equally with the premises; but it is difficult to see how the process of inference can be conceived as taking place in that form. Certainly no proof of it can then be given. If therefore the process of inference be so expressed it must be regarded as a symbolical process, symbolical of such an inference about things as has been described above, and it therefore seems to me more advisable to examine it under this latter form.

§ 8. The above, then, being the fundamental rules of inference in Probability, the question at once arises, What is their relation to the great body of formulæ which are made use of in treatises upon the science, and in practical applications of it? The reply would be that these formulæ, in so far as they properly belong to the science, are nothing else in reality than applications of the above fundamental rules. Such applications may assume any degree of complexity, for owing to the difficulty of particular examples, in the form in which they actually present themselves, recourse must sometimes be made to the profoundest theorems of mathematics. Still we ought not to regard these theorems as being anything else than convenient and necessary abbreviations of arithmetical processes, which in practice have become too cumbersome to be otherwise performed.

§ 9. This explanation will account for some of the rules as they are ordinarily given, but by no means for all of them. It will account for those which are demonstrable by the certain laws of arithmetic, but not for those which in reality rest only upon inductive generalizations. And it can hardly be doubted that many rules of the latter description have become associated with those of the former, so that in popular estimation they have been blended into one system, of which all the separate rules are supposed to possess a similar origin and equal certainty. Hints have already been frequently given of this tendency, but the subject is one of such extreme importance that a separate chapter must be devoted to its consideration.

§ 10. Before concluding this chapter the reader is reminded, in order to prevent misapprehension, that no assumption is made in the above remarks about the nature of demonstrative truth as involved in the rules of arithmetic. We have called them necessary rules, but it is quite immaterial for our present purpose whether they be derived from experience or not. The most strenuous assertor of their experimental origin will not deny that, as things now are, they are sharply marked off from mere inductive generalizations in respect of the strength of our convictions about their invariable truth. With our present mental constitution and experience the former

are irreversible and the latter generally are not, and this is abundantly sufficient to classify them apart. The discussion of such a question as this belongs, as do many other discussions, to the science of the laws of evidence and discovery in their most general form, rather than to such a limited portion of them as we are now occupied with.

CHAPTER V.

GENERAL REMARKS ON THE RESULTS OF THE FOREGOING CHAPTERS.

§ 1. As the remarks in the present chapter will be of a somewhat general character, it will be advisable to pause for a moment in order to obtain a clear conception of our present standing-point.

On the objective side, then, we have a series of events occurring in any order in time. This series of events is at bottom nothing but a series of groups of substances and attributes, to which groups various other attributes are found united, in a certain definite proportion of cases out of the whole. The existence of such a series is supposed to be known; by what evidence it may be established in any particular instance we are not called upon at present to enquire.

With regard to the subjective side, we must suppose some person, say myself, contemplating this series. I mentally single out some one or more of the individuals which compose the series, and endeavour to form an opinion, judging solely by the statistical frequency with which the attributes occur, whether in these selected instances the occasional attributes will

be present or not. There does not seem to be any better mode of expressing this than to say, that I form a *conception* or anticipation of some member of the series at present unknown to me, unknown at least in some of its characteristics; my conception includes in it, or excludes from it as the case may be, the occasional attributes, and in this respect I cannot of course feel certain about its being a correct representation of the facts. It is the duty of Probability to investigate with what degree of strength this conception should be entertained, in other words, how firmly we ought to believe it to be correct.

§ 2. Sometimes the member of the series thus singled out for anticipation may be supposed to be already within the certain grasp of experience in regard to some of its characteristics, our doubt and therefore the possible inaccuracy of our conception referring only to the remaining characteristics; sometimes it may be altogether unknown as yet, except as occupying a certain numerical position in the succession. In every case, however, we shall find that there is present to our minds a conception of an event which is at present tinged with doubt and which we are waiting to confirm or reject; Probability refers to this time of pause and doubt. For example, I know that four children in ten live to be fifty. Here the series is one of children, to four out of ten of whom we are able to assign the property of living to fifty. I select one,

my only remaining doubt is whether it will live to fifty. This may be expressed by saying that I form the conception of it as living to fifty, and want to ascertain how firmly I should entertain the conception. Or the child may as yet be no subject of experience, from its not being at present in existence. The child may be determined simply from its being, say, the first born next week. The process is precisely the same in all these instances. A conception is formed, and a value (*i.e.* amount of belief as explained in Ch. III.) assigned to it solely on statistical grounds.

§ 3. The above mentioned view of the subject is, I apprehend, the ordinary one involved in what is sometimes termed Material Logic. This view is not indeed so prominently brought before us there, but a little consideration will show that substantially the same view is involved in every science which professes to draw inferences about external things. In every such science we must suppose a certain number of facts given in experience, and therefore a certain number of propositions known to be true; the object aimed at in the inferences is to add to this domain of fact in every direction. It is not easy to see how this can be done except by forming conceptions, and then ascertaining whether these correspond to fact or not. It is true that this process is obscured in the case of those ordinary inferences which are supposed to amount to demonstration, for here the same infer-

ence which first suggests the conception to us may be the very thing which assures us of its truth. If so, the conception may be described as springing at once out of non-existence into the domain of fact. But whenever we are drawing conclusions about things by means of inductive rules which do not amount to demonstration, especially when the fact to be established depends upon a combination of several such arguments, we shall hardly be able to avoid taking the view now under discussion. In all such cases we have a multitude of conceptions (or whatever other name we may give to these notions in our minds) which we should be unwilling to call imaginary, and yet which we should scarcely be able, on the other hand, to speak of as facts. They are rather in a sort of noviciate, and qualifying for facts. But they are certainly at that moment present to us, and so far really existent in the mind. Our position, therefore, in these cases seems distinctly that of entertaining a conception, and the process of inference is that of ascertaining to what extent we are justified in adding this conception to the already-received body of truth and fact. This view of the subject is far more forcibly set before us in Probability than in any of the Inductive sciences, owing to the fact that in Probability we distinctly take notice of, and regard as evidence, reasons so faint that they would scarcely be called by any other name than mere hypotheses elsewhere. But

however slight may be the statistical grounds for believing in a thing, these grounds certainly suggest the conception of it to the mind, and they give some force which the mind can appreciate for believing in its truth.

§ 4. For additional clearness two brief remarks may be added. Let it be observed then that this is in no sense an adoption of the Conceptualist view of Logic. It would be so were we to set before us as our object to ascertain whether, for example, the conception 'dying after fifty years' is or is not involved in that of 'being born,' or with what amount of force we should believe it to be so contained, (were this last expression quite intelligible). But it is a very different thing to make out whether the former conception is or is not *true*, that is, whether it does or does not fit in with the rest of our experience about men. This latter is inference about things, and it is in this sense that Probability is understood in this Essay.

But, at the same time, when we speak about converting our conceptions into matters of fact, we do not at all imply any opinion as to whether these matters of fact are not at bottom resolvable into a collection of subjective impressions. This is a question with which we, as a kind of logicians, are in no way concerned. I may correct a person's impression of a steam-engine, for instance, or tell him that it is a

false one, without committing myself to any assertion as to whether all our experience of steam-engines can bring us to anything more at bottom than subjective impressions.

Keeping the foregoing remarks in mind, we shall easily see our way to several useful inferences.

§ 5. In the first place it will be seen that in Probability *time* has nothing to do with the question; in other words, it does not matter whether the event, whose probability we are discussing, be past, present, or future. The question, in its simplest form, is this:—Statistics (extended by Induction) inform us that a certain event has happened, does happen, or will happen, a certain proportion of times in a certain way. We form a conception of that event, and regard it as possible; but we want to do more; we want to know (from statistical data alone of course) *how much* we ought to expect it? (under the explanations already given about quantity of belief). There is therefore a sort of relative futurity about the event, inasmuch as our knowledge of the fact, and therefore our justification or otherwise of the correctness of our surmise, almost necessarily comes after the surmise was formed; but the futurity is only relative. The evidence by which the question is to be settled may not be forthcoming yet, or we may have it by us but only consult it afterwards. It is from the fact of the futurity being, as above described,

only relative, that I have preferred to speak of the conception of the event rather than of the anticipation of it. The latter term, which in some respects would have seemed more intelligible and appropriate, is open to the objection that it does rather, in popular estimation, convey the notion of an absolute as opposed to a relative futurity.

For example; a die is thrown. Once in six times it gives ace; if therefore we assume, without examination, that the throw is ace, we shall be right once in six times. In so doing we may, according to the usual plan, go *forwards* in time; that is, form our opinion about the throw beforehand, when no one can tell what it will be. Or we might go *backwards;* that is, form an opinion about dice that had been cast in time past, and then correct our opinion by the testimony of some one who had been a witness of the throws. In either case the mental operation is precisely the same; an opinion formed merely on statistical grounds is afterwards corrected by specific evidence. The opinion may have been formed upon a past, present, or future event; the evidence afterwards may be our own eye-sight, or the testimony of others, or any kind of inference; by the evidence is merely meant the subsequent examination of the case that is assumed to set the matter at rest. It is quite possible, of course, that this specific evidence should never be forthcoming; the conception in that case

remains as a conception, and never obtains that degree of conviction which qualifies it to be regarded as a 'fact.' This is the case with all past throws of dice, the result of which have not been recorded.

In discussing games of chance there are obvious advantages in confining ourselves to what is really, as well as relatively, future, for in that case direct information concerning the contemplated result being impossible, all persons are on precisely the same footing of ignorance, and must form their opinion entirely from the frequency of occurrence of the event in question. On the other hand, if the event be past, there is almost always evidence of some kind and of some value, however faint, to inform us what the event really was; if this evidence is not actually at hand, we can generally, by waiting a little, obtain something that shall be at least of use to us in forming our opinion. Practically therefore we generally confine ourselves, in anticipations of this kind, to what is really future, and so in popular estimation futurity becomes indissolubly associated with probability.

§ 6. But there is an error closely connected with this, or at least an inaccuracy of expression constantly leading to error, which has found large acceptance, and has been sanctioned by some writers of the greatest authority. Both Bishop Butler and Mr Mill have drawn attention to the distinction between im-

probability before the event and improbability after the event, which they assert to be perfectly different things; if however the principles laid down above be correct, such a distinction as this cannot be maintained.

Butler's remarks on this subject occur in his *Analogy*, in the chapter on miracles. Admitting that miracles are very improbable he strives to obtain assent for them by showing that other events, which are also very improbable, are received upon what is in reality very slight evidence. He says, "There is a very strong presumption against common speculative truths, and against the most ordinary facts, before the proof of them; which yet is overcome by almost any proof. There is a presumption of millions to one against the story of Cæsar, or of any other man. For suppose a number of common facts so and so circumstanced, of which one had no kind of proof, should happen to come into one's thoughts, every one would without any possible doubt conclude them to be false. And the like may be said of a single common fact."

It surely needs but little reflection to see that his illustration of his position completely overturns it. For is he not in reality speaking of two perfectly distinct things here? In the 'improbable thing before the proof' we have represented to us a man 'thinking of the story of Cæsar,' that is, forming a conception of

certain historical events, *without any grounds*, and speculating as to what value is to be attached to the probability of its truth. Such a conception is of course, as he says, rejected as utterly improbable. Now what does he understand by the 'improbability after the proof'? That a story not adopted at random, but actually suggested and supported by witnesses, should be true. This latter might be accepted; the former would undoubtedly be rejected; but all that this proves, or rather illustrates, is that the testimony of almost any witness is vastly better than a mere guess. We may in both cases alike speak of 'the event' if we will; but it should be clearly understood that what is really present to the man's mind, and what is to have its probable value assigned, is the conception of an event; and surely no two conceptions can have a much more important distinction put between them than that which is created by supposing one to be an unsupported guess, and the other the report of witnesses.

§ 7. Mr Mill, in a chapter of his Logic on the Grounds of Disbelief, speaks of persons making the mistake of "overlooking the distinction between (what may be called) improbability before the fact, and improbability after it, two different properties, the latter of which is always a ground of disbelief, the former not always." He instances the throwing of a die. It is improbable beforehand that it should

turn up ace, and yet afterwards, "there is no reason for disbelieving it if any credible witness asserts it." The introduction of the sentence, 'if any credible witness asserts it,' alters the whole question. So with his other example; 'the chances are greatly against *A. B.*'s dying, yet if any one tells us that he died yesterday we believe it.'

That the amount of our belief, in the above cases, has no necessary connection with the fact of the event being one which has already happened, or, as it is expressed, of the probability being *after* the fact, seems plain. Conceive for a moment, that some one had the power of knowing whether *A. B.* would die or not, (he might have some secret sources of knowledge unknown to ourselves). If he told us that *A. B.* would die to-morrow we should in that case be precisely as ready to believe him as when he tells us that *A. B. has* died. If we continued to doubt it would simply be because we thought that with him, as with us, the assertion rested on a guess and nothing more. So with the event when past; the fact of its being past makes no difference; until this credible witness tells us, we should doubt it if it came into our minds just as much as if it were future.

There is precisely the same distinction to be drawn in these examples as in those of Bishop Butler. What is really present to the man's mind is in one case a groundless conjecture (grounded only, that is,

on statistical information about the average), in the other the statement of a witness. The observer has in each case to assign its due value to the conception, and the conceptions being obtained in such different ways will naturally be valued differently.

§ 8. Butler's general argument in the chapter in question has been a good deal criticized. But his extraordinary opinion that every particular event is, when we come to think of it, excessively improbable, has attracted comparatively little notice. Connecting it with his other assertion, that these events are nevertheless established by slight evidence, we are forced to one of two alternatives. Either we are every day believing things which we have no grounds to believe, (this is adopting the common signification of the word improbable, as being nearly equivalent to deserving of little belief). Or on the other hand we must admit that the improbability of an event has little or no connection with the degree of our belief of it. The former alternative would almost effect a revolution in our belief, and the latter in our language.

It was apparently to avoid such a dilemma as this that Mr Mill has insisted upon the distinction between the probability before and after happening. He admits that the event would be improbable beforehand, but denies that it is so afterwards. Butler on the other hand admitted the event in both cases to be improbable, and yet claimed that in one case it could be easily

proved, with the object of course of obtaining equally easy credence for it in the other.

§ 9. If we bear in mind the distinction explained at the commencement of this chapter, we may see our way to a simple and satisfactory solution of the difficulty. We must remember that in strictness it is not *an event* which is improbable; it is to our conception of it only, or to the story in which the conception is conveyed, that this epithet can be applied; an event in itself can only be uncommon. I will refer for illustration to another example quoted by Mr Mill from Laplace. There is a lottery with 1000 tickets; it is therefore 999 to 1 against any particular number, say 79, being drawn. But now a witness whose veracity is but small, say $\frac{1}{10}$, comes and tells us that 79 has been drawn. By saying that a man's veracity is $\frac{1}{10}$ is meant that one in ten only of his statements are true. We conclude, therefore, on principles discussed in the preceding chapter, that the chance that he is speaking truth in this case is $\frac{1}{10}$. Here therefore it seems that we have found an instance in which an exceedingly improbable event is rendered moderately probable by means of the testimony of a witness of no extraordinary veracity. For it will most likely be maintained that the event in each case is precisely the same, namely the drawing of No. 79.

§ 10. But let us look a little closer. We shall then see that what is really 'improbable' in the former

case is our conception, that is, our guess, about the No. 79. We call it improbable in the first case because we are convinced that once only in a thousand times will such a guess be found to be correct. In the second case, also, the improbable thing is our conception about 79, but it here comes to us not as a guess but conveyed by a witness of given veracity. The ground it has to be called improbable is of the same kind as before, viz. because once only in ten times will it be found to be correct. But the mode in which the conception is obtained alters the amount of improbability exceedingly. Before, it was 999 to 1 against its being correct, now it is only 9 to 1.

§ 11. The distinction between these writers seems to be that in Mr Mill there is little more than an inaccuracy of expression; it does not appear that any directly erroneous inference has been made. Butler, however, meant exactly what he said; he was evidently satisfied with his principle, for he appeals to it again in his *Analogy*. He seems really to have believed that any proposition which was wildly improbable beforehand, was to be adopted afterwards the moment it was testified to by a generally trustworthy witness. He does not distinguish between a guess and an observation. To me the distinction between probability before and after the fact seems to resolve itself simply into this;—Before the fact we often have no better means of information than to guess one of

several possible alternatives, after the fact we often have, in addition to the guess, specific evidence; hence our estimate in the latter case is generally of more value. But if these characteristics were inverted, if, that is, we were to confine ourselves to guessing about the past, and if we could find any additional evidence about the future, the respective values of the estimates would also be inverted. The difference of these values has no connexion with time, but depends entirely upon the different grounds upon which our conception of the event in question rests.

§ 12. The origin of the mistake just discussed is worth enquiring into. I take it to be as follows. It is often the case as above remarked, when we are speculating about a future event, and almost always when that future event is a game of chance, that all persons are in precisely the same condition of ignorance in respect to it. The limit of available information is confined to statistics, and amounts to the knowledge that the unknown event must assume some one of various alternative forms. The conjecture therefore of any one man about it is as valuable as that of any other. But in regard to the past the case is very different. Here we are not in the habit of relying upon statistical information. Hence the conjectures of different men are of extremely different values; in the case of many they amount to what we call positive knowledge. This puts a broad distinction, in popular

estimation, between what may be called the objective certainty of the past and the future, which from the standing-point of a science of inference ought to have no existence.

In consequence of this, when we apply to the past and the future respectively, the somewhat ambiguous expression 'the chance of the event' it commonly comes to bear very different significations. Applied to the future it bears its proper meaning, namely, the value to be assigned to a conjecture upon statistical grounds. It does so, because in this case hardly any one has more to judge by than such conjectures. But applied to the past it shifts its meaning, owing to the fact that whereas some men have conjectures only, others have positive knowledge. By the chance of the event is now often meant, not the value to be assigned to a conjecture founded on statistics, but to such a conjecture derived from and enforced by any body else's conjecture, that is by his knowledge and his testimony.

§ 13. There is a class of cases in apparent opposition to some of the statements in this chapter, but which will be found, when examined closely, to confirm them in a remarkable manner. I am walking, say, in a remote part of the country and suddenly meet with a friend. At this I am naturally surprised. Yet if the view be correct that we cannot properly speak about events in themselves being probable or

improbable, but only of our conjectures about them, how do we explain this? We had formed no conjecture beforehand, for we were not thinking about anything of the kind, but yet few would fail to feel surprise at such an incident.

The reply might fairly be that we *had* formed such anticipations tacitly. On any such occasion every one unconsciously divides things into those which are known to him and those which are not. During a considerable previous period a countless number of persons had met us, and all fallen into the list of the unknown to us. There was nothing to remind us of having formed the anticipation or distinction at all, until it was suddenly called out into vivid consciousness by the exceptional event. The words we should instinctively use in our surprise seem to show this:—'Who would have thought of seeing you here?' viz. Who would have given any weight to the latent thought if it had been called out into consciousness beforehand? We put our words into the past tense, showing that we have had the distinction lurking in our minds all the time. We always have a multitude of such ready-made classes of events in our minds, and when a thing happens to fall into one of those classes which are very small we cannot help noticing the fact.

Or suppose I am one of a regiment into which a shot flies, and it strikes me, and me only. At this

I am surprised, and why? Our common language will guide us to the reason. 'How strange that it should just have hit *me* of all men!' We are thinking of the very natural two-fold division of mankind into, ourselves, and every body else; our surprise is again, at it were, retrospective, and in reference to this division. No anticipation was distinctly formed, because we did not think beforehand of the event, but the event, when it has happened, is at once assigned to its appropriate class.

§ 14. This view is confirmed by the following considerations. Tell the story to a friend, and he will be a little surprised, but less so than we were, *his* division in this particular case being,—his friends (of whom we are but one), and the rest of mankind. It is not a necessary division, but it is the one which will be most likely suggested to him.

Tell it again to a perfect stranger, and his division being different (viz. we falling into the majority) we shall fail to make him perceive that there is anything at all remarkable in the event.

I am not of course attempting in these remarks to justify our surprise in every case in which it exists. Different persons might be differently affected in the cases supposed, and the examples are therefore given mainly for illustration. Still on principles already discussed (Ch. III. § 30) we might expect to find something like a general justification of the amount of surprise.

§ 15. The answer commonly given in these cases, is confined to attempting to show that the surprise should not arise, rather than showing how it arises. It takes the following form,—'You have no right to be surprised, for nothing remarkable has really occurred. If this particular thing had not happened something equally improbable must. If the shot had not hit you or your friend, it must have hit some one else who was *à priori* as unlikely to be hit.'

For one thing this answer does not explain the fact that almost every one *is* surprised in such cases, and surprised somewhat in the different proportions mentioned above. So universal a tendency at least deserves to be accounted for; I have not seen any but that offered above that attempts to account for it.

But again, the answer has the inherent unsatisfactoriness of a dilemma. It admits that something improbable has really happened, but gets over the difficulty by saying that all the other alternatives were equally improbable. A natural inference from this is that there is a class of things, in themselves really improbable, which can yet be established upon very slight evidence. Butler accepted this inference, and worked it out to the extraordinary conclusion given above. Mr Mill attempts to avoid it by the consideration of the very different values to be assigned to improbability before and after the event. Some fur-

ther illustrations of this error will be found in the chapter on fallacies.

§ 16. In connection with the subject at present under discussion we will now take notice of a distinction which we shall often find insisted on in works on Probability, but to which apparently needless importance has been attached. It is frequently said that probability is *relative*, in the sense that it has a different value to different persons according to their respective information upon the subject in question. For example, two persons, A and B, are going to draw a ball from a bag containing 4 balls, A knows that the balls are black and white, but does not know more; B knows that three are black and one white. It will be said that the probability of a white ball to A is $\frac{1}{2}$, and to B $\frac{1}{4}$.

But surely there is nothing more in this than the principle, equally true in every other science, that our inferences will vary according to the data we assume. We might just as well speak of the area of a field or the height of a mountain being relative, and therefore having one value to one person and another to another. The real meaning of the example cited above is this; A supposes that he is choosing white at random out of a series which in the long run gives white and black equally often; B supposes that he is choosing white out of a series which in the long run gives three black to one white. By the application, therefore, of a

precisely similar rule they draw different conclusions; but so they would under the same circumstances in any other science. If two men are measuring the height of a mountain, and one supposes his base to be 1000 feet, whilst the other takes it to be 1001, they would of course form different opinions about the height. The science of mensuration is not supposed to have anything to do with the truth of the data, but assumes them to have been correctly taken; why should not this be equally the case with Probability?

§ 17. The former example, that of the balls and bag, appears plausible owing to the fact that two different persons, who had not looked into the bag, really might form different opinions about its contents. But if we take another example, in which the data are less mistakeable, we shall see how needless the assertion of the relativity of the probability becomes. And in most legitimate applications of Probability the data offer no more opportunity for difference of opinion than do those of any other science. A die, for example, is going to be tossed up. *A* supposes it to have six faces, *B* only five. Would it not seem somewhat frivolous to say that the probabilities of ace to the two men are respectively $\frac{1}{6}$ and $\frac{1}{5}$? The reply would be that we must at least assume them to have taken pains to arrive at correct data, and that a science cannot be called upon formally to recognize the erroneous

or groundless opinions of the observers. They must take this risk upon themselves. If Probability be confined to its proper province, no such distinction as the above would ever be needed or demanded; for in that case all persons may obtain the same statistical information if they choose to take the trouble, therefore knowledge inferior to this is not wanted. And they can none of them obtain anything more than these statistics, therefore superior knowledge is excluded. In other words, we shall naturally assume the observers to be in this, as in other sciences, all of them equally well-informed. If they are not, it is their own fault.

To describe two persons looking at the same bag, and to insist upon the different expectations which they entertain, and are bound on philosophical grounds to support, as to what will come out of it, is to make one of those too numerous applications of the theory of Probability which have served to bring an undeserved contempt upon the whole science.

CHAPTER VI.

THE RULE OF SUCCESSION.

§ 1. In a former chapter we discussed at some length the nature of that kind of inference in Probability which corresponds to one class of those termed in Logic immediate inferences. We ascertained what was the meaning of saying, for example, that the chance of any given man *A. B.* dying in a year is $\frac{1}{3}$, when concluded from the general proposition that one man out of three in his circumstances dies. But to stop at this point would be to take a very imperfect view of the subject. If Probability is a science of real inference about things, it must surely give us something more than immediate inferences; we must be able, by means of it, to step beyond the limits of what has been actually observed, and to draw conclusions about what is as yet unobserved. This leads at once to the question, What is the connection of Probability with Induction? This is a question into which it will be necessary to enter now with some minuteness.

§ 2. That there is a close connection between

Probability and Induction, must have been observed by almost every one who has treated of either subject; I have not however seen any account of this connection that seemed to me to be satisfactory. An explicit description of it should rather be sought in treatises upon the narrower subject, Probability, but it is precisely here that the most confusion is to be found. The province of Probability being somewhat narrow, incursions have been constantly made from it into the adjacent territory of Induction. In this way, amongst the arithmetical rules discussed in the last chapter but one, others have been introduced which, as I shall hope to show, ought not in strictness to be classed with them, as they rest on an entirely different basis.

§ 3. The origin of such confusion is easy of explanation; it arises, I think, from the habit of laying undue stress upon the *subjective* side of Probability, upon that which treats of the quantity of our belief upon different subjects and the variations of which that quantity is susceptible. It was seen that this variation of belief is at most but an invariable accompaniment of what is really essential to Probability, and is moreover common to other subjects as well. By defining the science therefore from this side these other subjects would claim admittance into it; some of these, as Induction, have been accepted, but others have been somewhat arbitrarily rejected. Our belief in a wider proposition gained by Induction is, prior

to verification, not so strong as that of the narrower generalization from which it is inferred. This being observed, a so-called rule of probability has been given by which it is supposed that this diminution of assent could in many instances be calculated.

§ 4. But *time* also works changes in our conviction; our belief in the happening of almost every event, if we recur to it long afterwards, when the evidence has faded from the mind, is less strong than it was at the time. Why are not rules of oblivion inserted in treatises upon Probability? If a man is told how firmly he ought to expect the tide to rise again, because it has already risen ten times, might he not also ask for a rule which should tell him how firm should be his belief of an event which rests upon a ten years' recollection? The infractions of a rule of this latter kind could scarcely be more numerous and extensive, as we shall see presently, than those of the former confessedly are. The fact is that the agencies, by which the strength of our conviction is modified, are so infinitely numerous that they cannot all be assembled into one science; for purposes of definition therefore the quantity of belief had better be omitted from consideration, and the science defined from the other or statistical side of the subject, in which, as has been shown, a clear boundary line can be traced.

§ 5. Induction, however, from its importance does merit a separate discussion; a single example

will show its bearing upon this part of our subject. We are considering the prospect of a given man, *A. B.*, living another year, and we find that nine out of ten men of his age do survive. In forming an opinion about his surviving, however, we shall find that there are in reality two very distinct causes which modify the strength of our conviction; distinct, but in practice so intimately connected that we are very apt to overlook one, and attribute the effect entirely to the other.

§ 6. (I) There is that which strictly belongs to Probability; that which (as was explained in Chap. III.) measures our belief of the individual proposition as deduced from the general. Granted that nine men out of ten of the kind to which *A. B.* belongs do live another year, it obviously does not follow that *he* will. We describe this state of things by saying, that our belief of his surviving is diminished from certainty in the ratio of 10 to 9, or, in other words, is measured by the fraction $\frac{9}{10}$.

(II) But are we certain that nine men out of ten like him *will* live another year? we know that they have in time past, but will they continue to do so? Since *A. B.* is still alive it is plain that this proposition is to a certain extent assumed, or rather obtained by Induction. We cannot however be as certain of the inductive inference as we are of the data from which it was inferred. Here, therefore, is a second cause which tends to diminish our belief; in practice

these two causes always accompany each other, but in thought they can be separated.

§ 7. The two distinct causes described above are very liable to be confused together, and the class of cases from which examples are generally drawn increases this liability. The step from the statement 'all men have died in a certain proportion' to the inference 'they will continue to die in that proportion' is so slight a step that it is unnoticed, and the diminution of conviction that should accompany it is unsuspected. In what are called *à priori* examples the step is still slighter. We feel so certain about the permanence of the laws of mechanics, that few would think it to be an inference when they believe that a die will in the long run turn up all its faces equally often, because other dice have done so in time past.

§ 8. It has been already pointed out (in Chapter III.) that, so far as regards the definition of Probability as the science which discusses the modification of our belief, the question at issue seems to be simply this. Are the causes alluded to in (II) capable of being reduced to one simple coherent scheme, so that any universal rules for the modification of assent can be obtained from them? If they are, strong grounds will have been shown for classing them with (I), in other words for considering them as rules of probability. Even then they might be rules of a different kind, contingent instead of necessary, but this objection

might perhaps be overruled by the greater simplicity secured by classing them together. This view is, with various modifications, almost universally adopted by writers on Probability. Or, on the other hand, must these causes be regarded as a vast system, one might almost say a chaos, of perfectly distinct agencies; which may indeed be classified and arranged to some extent, but from which we can never hope to obtain any rules of wide generality which shall not be subject to constant exception? If so, but one course is left; to exclude them all alike from Probability. In other words, we must assume the general proposition, that which has been described throughout as our starting-point, to be given to us; it may be obtained by any of the numerous rules furnished by Induction, or it may be inferred deductively, or given by our own observation; its value may be diminished by its depending upon the testimony of witnesses, or being recalled by our own memory. Its real value may be influenced by these causes or any combinations of them; but all these are preliminary questions with which we have nothing directly to do. We assume our statistical proposition to be true, neglecting the diminution of its value by the process of attainment; we take it up first at this point and then apply our rules to it. We receive it in fact, if one may use the expression, *ready-made*, and ask no questions about the process or completeness of its manufacture.

§ 9. It is not to be supposed, of course, that any writers have seriously attempted to reduce to one system all the causes mentioned above, and to embrace in one formula the diminution of certainty to which the inclusion of them subjects us. But on the other hand, they have been unwilling to restrain themselves from all appeal to them. From the first study of the science attempts have been made to proceed by the Calculus of Probability from the observed cases to adjacent and similar cases. In practice, as I have already said, it is not possible to avoid some extension of this kind. But it should be observed, that in these instances the divergence from the strict ground of experience is not in reality recognized; we have, it is true, wandered somewhat from it, and so obtained a wider proposition than our data, and therefore one of less certainty. Still we assume the two to be equally certain, or rather omit all notice of the divergence from consideration. It is assumed that the unexamined instances will resemble the examined; the theory of the calculation rests upon the supposition that there will be no difference between them, and the practical error is insignificant simply because this difference is small.

§ 10. But the rule we are now about to discuss, and which may be called the Rule of Succession, is of a very different kind. It not only recognizes the fact that we are leaving the ground of past experience,

but takes the consequences of this divergence as the express subject of its calculation. It professes to give a general rule, of unlimited application, for the measure of expectation that we should have of the reappearance of a phenomenon that has been already observed any number of times. This rule is generally stated somewhat as follows: "To find the chance of the recurrence of an event already observed, divide the number of times the event has been observed, increased by one, by the same number increased by two."

§ 11. It must be confessed that this rule has been received in a thankless spirit. For considering what a number of events there are in the world, and how many are the ways in which they may happen, there is certainly no reason to fear that there will long be any want of occasions on which to appeal to the rule. The truth of it does not seem to be doubted by any of the writers on Probability; whilst those who have obtained their results from the mathematicians, as Archbishop Thomson, in his *Laws of Thought*, seem to regard it as standing on precisely the same footing as any of the other rules of the Science. It is our task at present to examine its claims to acceptance.

§ 12. We will begin with a detailed criticism of this rule. This is necessary both from the general acceptance it has received, and from the eminence of many of its supporters. Moreover, however much the rule itself may be found to fail when examined, many

principles of inference of real intrinsic importance will, I hope, be elicited in the course of our investigation. We will afterwards shift the enquiry on to somewhat broader grounds, and examine some of the problems which have to be met in the case of any attempt to lay down rules of proof and discovery. These enquiries do not very strictly belong to Probability, but they are so constantly encountered there that it seems essential to clear our way towards forming a decided opinion upon them.

§ 13. Now there is one view of the question which deserves a passing notice, but nothing more. We may presume that the eminent writers who have accepted this rule do not regard it as the expression of a mere brute instinct. It is conceivable that on Physiological or Psychological grounds, the mere repetition of an event a certain number of times should excite a growing expectation of its recurrence. Employing an illustration that shall at least have some connection with the derivation of the word, our impressions might be like those produced by hitting a soft plank with a mallet; each successive blow deepens the impression. This would be to regard the rule as merely a mental law or instinct. But if it is to be considered as supplying real inferences about things, we cannot rest here. An instinctive belief may need to be corrected, to bring it into accordance with experience; and if not, it must at least submit to

justify itself by experience. It has been repeatedly stated already that to tell a rational being that his expectation of an event should be, say, $\frac{3}{4}$, can mean nothing else at bottom than this;—that events of the kind contemplated do really happen in that way three times out of four.

§ 14. Is the rule then really true? Let us appeal to experience. In order that there shall be no unfairness we will begin with one or two examples selected by some of the most eminent writers on the subject. Quetelet informs us, that the man who has seen the tide rise ten days successively is right in entertaining an expectation of $\frac{1}{1\,2}$ that it will do so again. Laplace has ascertained that, at the date of the publication of his work, one might have safely betted 1826214 to 1 in favour of the sun's rising again. Since then however time has justified us in laying longer odds. De Morgan says, that a man who standing on the bank of a river has seen ten ships pass by with flags, should judge it to be 11 to 1 that the next ship will also carry a flag. Let us add an example or two more of our own. I have observed it rain three times successively,—I have found on three separate occasions that to give my fowls strychnine has caused their death,—I have given a false alarm of fire on three different occasions and found the people come to help me each time. In each of these cases, then, I am to form an opinion of just the intensity of $\frac{4}{5}$ in favour of

a repetition of the phenomenon under similar circumstances. But no one, we may presume, will assert that in any one of these cases the opinion so formed would be correct. In some of them our expectation would have been overrated, in some immensely underrated. By calling the expectation wrong, it is not merely meant that it is frustrated in the particular case in question; this kind of failure, of course, is to be looked for in questions of Probability. But it is wrong in the long run. No amount of repetition in our appeals to the rule in similar cases would lead us to even an average truth; this latter kind of truth we have a right to look for. With one single exception, and that a very doubtful one (the case of games of chance, bags and balls, &c.), the same objection could probably be brought against every possible application of this rule. Now granting that a formula of this kind, being given to any one, he might be justified in making use of it for a time, surely as soon as he has tried it and repeatedly found it lead him astray, it becomes his duty to reject and denounce it for the future. If, in the above examples, a person has really proportioned his expectation in the manner described, and has afterwards discovered by the examination of other instances of the same class, as he could hardly fail to do, that his opinion had been grossly wrong, is he still to adhere to the rule for the future? We need not blame him for doing what he did at the time;

he might have known no better; but is he to let the rule be published to the world as a true one?

§ 15. It is merely evading the difficulty to assert, as is sometimes done, that the rule is to be employed in those cases only in which we do not know anything beforehand about the mode and frequency of occurrence of the events. The truth or falsity of the rule surely cannot be in any way dependent upon the ignorance of the man who uses it. His ignorance affects himself only, and corresponds to no distinction in the things. In reality the two classes, viz. of cases in which we have and have not some preliminary information, are for the most part identical; not, of course, identical to the same person at the same time, but in the sense that what one person does not know at present, he may hereafter, and others do know now. To say therefore that the rule refers to cases where there is no such preliminary information, is irrelevant when the question is as to the correctness of the rule. We cannot fling the rule amongst mankind with the prescription attached that it is merely to be taken by the ignorant. They might have been inclined to accept it once, but as soon as they know that its truth is denied by the better-informed, this amount of knowledge, though they possess no more, will be quite sufficient to prevent them from trusting to the rule.

§ 16. I have said that the truth of the Rule of

Succession seems never to be doubted by mathematical writers on the subject. From this statement however exceptions must be made. Prof. De Morgan is far too acute and philosophical a writer to accept the rule with the blind confidence with which it has sometimes been received. He regards it as furnishing a *minimum* value for the amount of our expectation.

He would appear therefore to recognize only the instances in which our belief in the uniformity of nature, and in the existence of special laws of causation, comes in to aid that which we should entertain from the mere frequency of past occurrence of the particular event in question. His opinion is one from which I would dissent with deference, but it certainly appears to me to be irreconcilable with some of the instances given above. We have seen that there are cases in which the fact of a thing having already happened several times is a strong reason *against* its happening again. Can any marks be given by which these particular cases should be detected beforehand? and, if not, how can we assign a minimum value to the formula? A false alarm given several times in succession is no unfair specimen of a considerable class of recurrences (others will be given in Chap. XII). Whilst such cases exist I cannot see that the rule can be regarded as correct.

§ 17. It will not save the credit of the rule, in the above instance, to attempt to find its justification

in some broader generalization, which controls and supersedes the narrower; to say, for example, that the measure of our expectation that the event will *not* recur is assigned by the number of times in which it has thus ceased to recur at that point before. This is shifting the ground, and instead of proving the correctness of the rule in question, offering to prove that of some other rule instead. It is like saying, in justification of some law which is accused of being pernicious, that the constitution in accordance with which this law was framed is itself on the whole highly advantageous. If we were speaking of the broader generalization, such a remark would be to the point, but we are not.

The rule of succession informs us that, when an event has happened in a certain way four times, it is 5 to 1 (some say *at least* 5 to 1) that it happens so next time. Against this we may adduce, not merely single cases, but whole classes of cases, in which such an opinion would be grossly erroneous. Each of these, of course, is a succession, and therefore has quite as much claim as any other succession to be included within the rule; it does not save the credit of the rule to say that it applies to another and quite different succession. How is any one to know beforehand whether it applies to his own particular circumstances or not, unless it be, as of course it professes to be, perfectly general?

§ 18. When we look at the above more closely we shall find, I think, that it is really a defence of the rule, if defence it can be called, which, at the absolute sacrifice of its validity as a rule strictly so termed, would seek to retain and justify it as a general principle. Admitting it to be true that the rule fails, if we infer, from the fact of its having rained three days running, that it will rain again, what else in fact are we doing, it may be said, but abandoning the rule in one form to retain it in another? If we experience another such succession of three rainy days, we now do not expect it to be followed by a fourth of the same kind; in so doing, it may be urged, are we not necessarily resorting to the very rule that we professed to discard? are we not now making a precisely similar succession, not indeed of individual rainy days, but of *successions* of rainy days, and so forming our anticipation?

§ 19. Whatever might be said for the above defence, it is fatal, as I have already remarked, to the integrity and utility of the rule, as a rule. To follow up such an enquiry as that to which it seems to conduct us, would be to wander far from the province of Probability. It would lead to an investigation, not exactly into the direct formation of rules of inference, and certainly not into their correctness, but into the ultimate principle or constitution of the mind upon which all Induction is founded. It would

involve, as it appears to me, a discussion of the fundamental laws of association, upon which all inferences about things might be conceived ultimately to rest; a discussion which would belong more properly to Psychology than to any branch of Logic.

§ 20. It is quite true that there must be some mental link to bind together the examined and the unexamined cases, before we can make any new inferences about the latter. Without such a link there could of course be no extension beyond the strict limits of past experience. It is also obvious that a stronger degree of conviction or anticipation is produced in some instances than in others, the strength of this conviction depending unquestionably, in part, upon the degree of resemblance between the examined and the unexamined cases; it may also depend in part upon the number of times in which the examined cases have been already observed. But it need not be the case, as seems to be commonly supposed, that the strength of conviction must increase uniformly from zero towards certainty in proportion to the number of these observed instances of recurrence. It is at least equally possible, as is held by some writers, that the conviction should exist in its full degree after the *first* occurrence of the event, viz. that our primitive impulse should be to fully believe that any two things which had been once observed together would so occur again; this belief becoming of course

altered in amount and often removed by subsequent experience.

But whichever way the matter be settled it is not easy to see how such considerations can have any bearing upon rules of inference at the point at which they are taken up in Logic or Probability. The psychological principles just mentioned lie at an immense depth below the surface of these rules, and assume a very different form before they emerge into the shape of laws of inference for minds of mature intelligence. In this latter shape they must, of course, submit to be tested by experience, as we have tested them throughout; but I cannot see that we are concerned with the process of their growth, or the germ out of which they have been developed.

I find it difficult to ascertain precisely from Laplace's Essay what his view of this Rule of Succession is. On the one hand he certainly appeals to it as a valid rule of inference, but on the other hand he enters into decidedly psychological and even physiological explanations in the latter part of his Essay. But he does not appear to perceive the fact that by converting any such formula into an ultimate principle we do in reality abandon it as a practical rule.

§ 21. But if this rule be regarded strictly as a rule, the reader may well be supposed to enquire, by this time, how it was ever discovered, and whence it obtains its proof? We have not far to seek for in-

formation upon these points. It certainly was not discovered from observation or experience of nature, for this, as we have seen, contradicts it in almost every instance. Nor was it discovered by observation of the mind; for this only leading to a knowledge of what men do believe, and not of what they should believe, can be no valid guide in drawing inferences about things without us.

There seems to remain but one way. We may discover amidst the infinite complexity of nature some class of objects that may be regarded as a fair type or sample of all the rest. The play of the different agencies at work elsewhere may be there laid bare to view, as it were, so that we may feel certain that so far as regards the succession of phenomena we have arrived at some of the fundamental principles of the universe. It is obvious that the connection between this class of objects and the rest of nature must be of no transient or superficial character. But when we have discovered this connection we shall be able to infer a rule of such broad generalization that in no single instance will any man be able to act upon it. Such an example has been discovered by some of the supporters of this rule. What then are the data by which this grand generalization is drawn? by which, according to Laplace, we feel a confidence, as the sun sets, of more than a million to one that it will rise again? and by which each generation of husbandmen

may go on sowing and reaping with a deeper persuasion than their fathers possessed before them, that seed-time and harvest and summer and winter will not fail? A study of the works of these writers will discover that it is a bag containing balls of a black and white colour. Rules, of more or less accuracy, are established as to the surmises we may form about the proportion of different colours in the bag, after we have drawn a few, and therefore of the proportion that will continue to be given in future. The supposition apparently slips in somewhere about here that the universe is constructed on the same principle as such a bag, from which the rule, in all its generalization, is supposed at once to follow.

The above is no caricature of the process by which this Rule of Succession is commonly obtained. If there are any persons who believe that something of greater value than formulæ for the manipulation of symbols can be obtained in this way, they should mark the two following chasms in the logic, across neither of which is it easy to find a passage. The first lies between the premises and conclusion of the argument by which we infer from a limited number of drawings what was the number of balls of each colour in the bag. This will be referred to again in Chap. VIII. The second lies in our way when we try to proceed from such a rule as this about balls to somewhat more general conclusions about the phenomena of nature.

§ 22. It is worth pausing for a moment in order to understand the nature of the rule if it be supposed to be obtained in this way. It need not lead us to truth in any single case; this of course is not expected in Probability. Nor need it lead us to truth in the average of any class of cases; this might fairly have been expected. But the rule will profess that an appeal to it in all classes of cases whatever, by all mankind, would lead to truth, and that it is ready to submit to this test of universal and incessant experience. On such a view the rule seems almost equally to evade attack and defence; its vagueness and generality are its protection. With equal reason might we attempt to take the average size of all measurable things, and then determine to act upon the assumption that every thing is just of that size. We might conceive a sort of justification of the average conduct of all mankind if they always acted so; but how would any one person prosper during a limited time on these terms? Any rule of discovery that Inductive Logic can recognize must surely be specialized by the imposition of some limits of time and place to its applicability.

§ 23. The Rule has been discussed, during this chapter, in its simplest form. Our criticisms however will apply with equal or greater force to the more complex form, in which it is attempted to determine

the chance, not of one more recurrence only, but of any number of recurrences.

§ 24. So much then for this Rule of Succession. Not that we have yet exhausted its shortcomings; for, as we shall see presently, it does not merely mislead us by giving one determinate but incorrect answer; it perplexes us by the offer of several discordant and often contradictory answers, all of them incorrect. Nothing but the celebrity of its supporters, and the general acceptance it has met with, have been our reasons for examining it so minutely as we have done.

But to simply criticise and reject it is not sufficient. We should like to know somewhat more fully *why* it fails so utterly, and whether anything can be substituted for it, for the end it seeks, viz. the extension of our inference beyond the limits of direct observation, is one which is desirable and necessary if we are ever to obtain information about things in general. The rule, as has been already said, seems to involve considerable confusion between Probability and Induction. This confusion can only be resolved, and the portion of truth mixed up in it elicited, by trying back some steps, and commencing with an analysis of the province and nature of Induction. This is a process which will be entered on in the next chapter.

CHAPTER VII.

INDUCTION, AND ITS CONNECTION WITH PROBABILITY.

§ 1. A RULE was examined at some length in the last chapter, the object of which rule was to enable us to make inferences about instances as yet unexamined. It was professedly, therefore, a rule of an inductive character. But, in the form in which it is commonly expressed, it was found to fail utterly, proving, when applied to the phenomena of nature, to be generally at least false or inapplicable. It is reasonable therefore to enquire at this point whether Probability is entirely a formal or deductive science, or whether, on the other hand, we are able, by means of it, to make valid inferences about instances as yet unexamined. This question has been already in part answered at the commencement of the last chapter. I propose in the present chapter to give a fuller investigation to this subject, and to describe, as minutely as limits will allow, the nature of the connection between Probability and Induction. We shall find it advisable for clearness of conception to commence our enquiry at a somewhat early stage. We will travel over the ground however as rapidly as

possible until we approach the boundary of what can properly be termed Probability.

§ 2. Let us then conceive some one setting to work to investigate nature, under its broadest aspect, with the view of systematizing the facts of experience that are known, and thence discovering others which are at present unknown. He observes a multitude of phenomena, physical and mental, contemporary and successive. He enquires what connections are there between them? what rules can be found, so that some of these things being observed I can infer others from them? We suppose him, let it be observed, deliberately to investigate the things themselves, and not to be turned aside by any prior enquiry as to there being laws under which the mind is compelled to judge of the things. This may arise either from a disbelief in the existence of these mental laws, and a consequent conviction that the mind is perfectly competent to observe and believe anything that experience offers, and should believe nothing else, or simply from a preference for investigations of the latter kind. In other words, we suppose him to reject Formal Logic, and apply himself to a study of objective existences.

§ 3. His task at first might be conceived to be a slow and tedious one. It would consist of a gradual accumulation of individual instances, as marked out and connected together by resemblances; these would

then be summed up in general propositions, from which inferences could afterwards be drawn. These inferences could, of course, contain no new facts, they would only be repetitions of what he or others had previously observed. The principles of ordinary logic would of course be needed now, but these would rather be regarded as being determined by the constitution of the things than by that of the mind in observing the things. So far we have supposed the observer not to have advanced beyond the province of applied logic in its usual sense.

§ 4. But a very short course of observation would suggest the possibility of a wide extension of his information. Experience itself would soon detect that events were connected together in a regular way; he would ascertain that there are 'laws of nature.' Coming with no *a priori* necessity of believing in them, he would soon find that as a matter of fact they do exist, though he could not feel any certainty as to the extent of their prevalence. The discovery of this arrangement in nature would at once alter the plan of his proceedings. His main work now would be to find out by what means he could best discover these laws of nature.

An illustration may assist. Suppose I were engaged in breaking up a vast piece of rock, say slate, into small pieces. I should begin by wearily working through it inch by inch. But I should soon find the

process completely changed owing to the existence of *cleavage*. By this arrangement of things a very few blows would do the work,—not, as I had at first supposed, to the extent of a few inches,—but right through the whole mass. In other words, by the process itself of cutting, as shewn in experience, and by nothing else, a constitution would be detected in the things that would make that process vastly more easy and extensive. Such a discovery would of course change our tactics. Our principle object would thenceforth be to ascertain the extent and direction of this cleavage.

Something resembling this is found in Induction. The discovery of laws of nature enables the mind to dart with its inferences from a few facts completely through a whole class of objects, and thus to acquire results the successive individual attainment of which would have involved long and wearisome investigation. We have no demonstrative proof that this state of things is universal; but having found it prevail extensively, we go on with the resolution at least to try for it everywhere else, and we are not disappointed. From propositions obtained in this way, or rather from the original facts on which these propositions rest, we can make *new* inferences, not indeed with absolute certainty, but with a degree of conviction that is of the utmost practical use. We have gained the great step of being able to make trust-

worthy generalizations. We conclude, for instance, not merely that John and Henry die, but that all men die.

§ 5. The above investigation contains, I think, a tolerably correct outline of the nature of the Inductive inference, as it presents itself in Material or Phenomenalist Logic*. It involves the distinction drawn by Mr Mill, and with which the reader of his System of Logic will be familiar, between an inference drawn *according* to a formula and one drawn *from* a formula. We do in reality make our inference from the data afforded by experience directly to the conclusion; it is a mere arrangement of convenience to do so by passing through the generalization. But it is one of such extreme convenience, and one so necessarily forced upon us when we are appealing to our own past experience or to that of others for the grounds of our conclusion, that practically we find it the best plan to divide the process of inference into two parts. The first part is concerned with establishing the generalization; the second (which contains the rules of ordinary logic) determines what conclusions can be drawn from this generalization.

§ 6. We may now see our way to ascertaining the province of Probability and its relation to kindred sciences. Inductive Logic gives rules for discovering

* I have borrowed this latter term from Professor Grote's admirable and suggestive *Exploratio Philosophica*.

such generalizations as those spoken of above, and for testing their correctness. If they contain universal propositions it is the part of ordinary logic to determine what inferences can be made from and by them; if, on the other hand, they contain proportional propositions, that is, propositions of the kind described in our first chapter, they are handed over to Probability. We find, for example, that three infants out of ten die in their first year. It belongs to Induction to say whether or not we are justified in generalizing our observation into the assertion, All infants die in that proportion. When such a proposition is obtained, whatever may be the value to be assigned to it, we recognize in it a series of a familiar kind, and it is at once claimed by Probability.

In this case the division into two parts, the inductive and the ratiocinative, seems something more than one of convenience; it is imperatively necessary for clearness of thought and arrangement. It is true that in almost every example that can be selected we shall find both of the above elements existing together and combining to modify the degree of our conviction, but when we come to examine them closely it appears to me that the grounds of their cogency, the kind of conviction they produce, and consequently the rules which they give rise to, are so entirely distinct that they cannot possibly be harmonized into a single consistent system.

The common opinion therefore which regards Inductive formulæ as composing a portion of Probability, and which finds utterance in the Rule of Succession criticised in our last chapter, cannot, I think, be maintained. It would be more correct to say, as stated above, that Induction is quite distinct from Probability, but yet co-operates with almost all its inferences. By the former we determine, for example, whether we can safely generalize the proposition that four men in ten live to be fifty; supposing such a proposition to be generalized, we hand it over to Probability to say what sort of inferences can be deduced from it.

§ 7. We may now see clearly the reasons for the limits within which causation* is necessarily required, but beyond which it is not needed. To generalize a formula so as to make it extend from the known to the unknown, it is clearly essential that there should be a certain permanence in the order of nature; this permanence is one form of what we mean by causation. If the circumstances under which men live and die remaining the same, we could not infer that four men out of ten would continue to live to fifty, because in the case of those whom we had observed this proportion had hitherto done so, it is clear that we should be admitting that the same antecedents need not be

* A separate chapter will be devoted to the discussion of Causation.

followed by the same consequents. This uniformity being what the Law of Causation asserts, the truth of the law is clearly necessary to enable us to obtain our generalizations; in other words, it is necessary for the Inductive part of the process. But it is equally clear, I think, that causation is not necessary for that part of the process which belongs to Probability. Provided only that the truth of our generalizations is secured to us, in the way just mentioned, what does it matter to us whether or not the individual members are subject to causation? For it is not in reality about these individuals that we make inferences. As this subject is more fully treated in another chapter, I need not make any further allusion to it here.

§ 8. The above description of the process of obtaining these generalizations must suffice for the present. Let us now turn and consider the means by which we are practically to make use of them when they are obtained. The point which we had reached in the course of the investigations entered into in the fourth chapter was this:—Given a series of a certain kind, we could draw inferences about the members which composed it; inferences, that is, of a particular kind, the value and meaning of which were fully discussed in their proper place.

§ 9. We must now shift our point of view a little; instead of starting, as in the third chapter, with a determinate series supposed to be given to us, let us

assume that the individual only is given, and that the work is imposed upon us of finding out the appropriate series. How are we to set about the task? Before our data were of this kind:—Eight out of ten men, aged fifty, will live ten years more, and we ascertained in what sense, and with what certainty, we could infer that, say, John Smith, aged fifty, would live ten years.

Let us now suppose, instead, that John Smith presents himself, how should we in this case set about obtaining a series? In other words, how should we collect the appropriate statistics? It should be borne in mind that when we are attempting to make real inferences about things as yet unknown, it is in this form that the problem will practically present itself.

The answer to this question may seem to be obtained by a very simple process, viz. by counting how many men of the age of John Smith, respectively do and do not live for ten years. In reality however the process is far from being so easy as it appears. For it must be remembered that each individual has not one distinct and appropriate series, to which, and to which alone, it properly belongs. We may indeed be practically in the habit of considering it under such a single aspect, and it may therefore seem to us more familiar when it occupies a place in one series rather than in another, but such a practice is merely customary on our part, not essential. It is obvious that

every individual thing or event has an indefinite number of properties or attributes observable in it, and might therefore be considered as belonging to an indefinite number of different classes of things. By belonging to any one class it of course becomes at the same time a member of all the higher classes, the genera, of which that class was a species. But, moreover, by virtue of each attribute which it possesses, it becomes a member of a class not necessarily conterminous with any of the other classes. John Smith is a consumptive man say, and a native of a northern climate. Being a man he is of course included in the class of vertebrates, also in that of animals, as well as in any higher classes that there may be. The property of being consumptive refers him to another class, narrower than any of the above; whilst that of being born in a northern climate refers him to a totally distinct class, not conterminous with any of the rest, for there are things born in the north which are not men. Now when he stands before us as an individual, it is altogether arbitrary under which of these aspects we view him, whilst the conveyance of our opinion to others depends on the name by which we call him. I do not mean of course that we need know of all these properties so as to be acquainted with the corresponding classes; it is quite sufficient that in any ordinary state of knowledge and intelligence we are familiar with some such properties, and recognize in conse-

quence that the individual may be referred to several different classes. To the student of logic all this will be too familiar to need explanation.

This variety of classes to which the individual may be referred, owing to its possession of a multiplicity of attributes, has an important bearing on the process of inference which was touched upon in the earlier sections of this chapter, and which we are now about to examine more in detail.

§ 10. It will serve to bring out more clearly some of the peculiarities in the case of Probability, of the step which we are now about to take, if we first examine the form it assumes in the case of ordinary Logic. Suppose, then, that I wish to ascertain whether a certain man, who is amongst other things a consumptive pauper, aged thirty, will live twenty years more. The terms in which the man is thus introduced refer him to different classes, in the way just described. Corresponding to these classes there will probably be a number of propositions discovered by previous observations or Inductions. They may be such as these following;—Some men live to sixty. No consumptives live to forty. No pauper lives to fifty. At the stage of enquiry which we at present occupy we may of course suppose any number of such propositions that we may need to be ready at hand. It need hardly be said that those given here do not profess to be true, but are only chosen for illustration.

From the first of these propositions we can infer nothing to our purpose. By either the second or the third we can answer our enquiry. To the logical reader it need not be pointed out that the process now under consideration is that of finding middle terms which shall embrace the individual in question.

Connected with the above logical process there are two considerations to which the reader's attention is especially directed; Firstly, if we can infer anything at all, we do so absolutely; assuming our premises to be true, we either know our result for certain, or know nothing about it. Secondly, no one of the above possible major propositions can ever contradict the others, or be to any extent at variance with them. To suppose this possible would be in effect to make two contradictory assumptions about matters of fact.

§ 11. But now observe the difference when we attempt to take the corresponding step in Probability. For the question stated above, substitute the following:—Will the man in question live *one* year? We shall find no universal propositions here, but we may find an abundance of proportional propositions. They will be of the following description (the data are purely imaginary):—Of men aged thirty, ninety-nine in a hundred live to thirty-one; of paupers of that age, nineteen in twenty survive one year; of consumptive men, but one in three survive. In both

of the respects to which attention was drawn, propositions of this kind offer a marked contrast to those last considered; they do not assert unequivocally yes or no, but give what in the case of the individual is a kind of qualified or hesitating answer. And these answers, though they cannot formally contradict one another, may yet be more or less in conflict.

§ 12. Hence it is obvious that in the attempt to draw a conclusion we may be placed in a position of some perplexity, but it is a perplexity that may present itself in two forms, a mild and an aggravated form.

The mild form occurs when the different classes referred to above are successively included one within another, for here our sets of statistics, though different, will be found scarcely, if at all, at variance with one another. The only difference between one set and another is that as we ascend in the scale to the larger genera the statistics become less appropriate, and the information they afford, therefore, less explicit and accurate. Let us examine this case first. What we originally wanted to ascertain, be it remembered, is whether John Smith will die within one year. But all knowledge of this fact being unattainable, we felt justified (under the restrictions mentioned in Chapter III.) in substituting as the best available equivalent for such information, the

following *statistical* enquiry, What proportion of men in his circumstances die?

But then there arises some doubt and ambiguity as to what exactly is to be understood by his circumstances. Knowing well what they are in themselves we are in perplexity as to how many we ought to take into account. We might confine our attention to those only which he has in common with all animals; if so we should have such statistics as this, ninety-nine animals out of a hundred will die within a year. But we reject this and prefer a narrower series, for the obvious reason that by so doing we secure our being more often right*, and, when we are not right, of being less flagrantly in error.

The above reasons are conclusive against taking too wide a class, but how can our class be too narrow? John Smith is not only an Englishman, he may be also from Suffolk, a farmer, &c. Why do we also reject these narrower classes? We do reject them, but it is for what may be called a practical rather than a theoretical reason. It must be borne in mind (as was shown and illustrated in the first chapter),

* More often right, that is, when we make a succession of such judgments about *men*. Some predetermination, not necessarily as to the particular class, but at least as to the sort of class to which we mean to direct our attention, cannot be avoided. In other words, we must assume the existence not only of a system of classification, but of certain channels in which our judgments about the objects included in these classes principally lie.

that it is essential to the sort of series we want that it should contain a considerable number of terms. Now many of the attributes of any individual are so rare that to take them into account would be at variance with this fundamental position of our science, that we are properly only concerned with the averages of large numbers. The more special the statistics the better, if we could only get enough of them, and so make up the requisite large numbers, but this is unfortunately impossible. We are therefore obliged to neglect one attribute after another, at the probable risk of increased inaccuracy, for at each step of this kind we diverge more and more from the sort of instances that we really want. We make our stand finally at the point where we can first obtain statistics drawn from a sufficiently large range of observation.

§ 13. In such an example as the one mentioned above, where one of the classes—man—is a natural kind, there is such a complete break at this point, that on the one hand no one would ever think of introducing any reference to the higher classes with fewer attributes, such as animal or organized being. And on the other hand the inferior classes, such as farmer or inhabitant of Suffolk, do not differ sufficiently in their characteristics from the class *man* to make it worth our while to attend to them. Now and then indeed these characteristics do become impor-

tant, and whenever this is the case we concentrate our attention upon the class to which they correspond, that is, the class which is marked out by their possession. Thus the quality of consumptiveness separates any person off from his fellow-men so widely that statistics about the lives of consumptive men would differ materially from those which refer to men in general. And we see the result; if a consumptive man can effect an insurance at all, he must do it for a much higher premium, calculated upon his special circumstances. In other words, the attribute is sufficiently important to mark off a fresh series.

§ 14. Where the classes thus correspond to natural kinds the process is not difficult; there is almost always some one of these kinds which is so universally recognized to be the appropriate one that most persons are quite unaware of there being any necessity for a process of selection. Except in the cases where a man has a sickly constitution, or follows a dangerous employment, we never think of collecting statistics for him from any class but that of men in general of his age in the country.

When, however, these successive classes are not ready marked out for us by nature, and thence arranged in easily distinguishable groups, the process is more obviously arbitrary. Suppose we were considering the chance of a man's house being burnt down, with what collection of attributes should we

rest content in this instance? Should we embrace all kinds of buildings, or only dwelling-houses, or confine ourselves to those where there is much wood, or those which have stoves? All these attributes, and a multitude of others may be present, and, if so, they are all circumstances which help to modify our judgment. We must be guided here by the statistics which we happen to be able to obtain in sufficient numbers. Rough distinctions of this kind are practically drawn in Insurance offices, by dividing risks into ordinary, hazardous, and extra-hazardous. We examine our case, refer it to one or other of these classes, and then form our judgment upon its prospects by the statistics appropriate to its class.

So much for what I have called the mild form in which the ambiguity occurs; but there is an aggravated form in which it may show itself, and which seems to place us in far greater perplexity.

§ 15. Suppose that the different classes mentioned above are not included one within the other. We may then be quite at a loss which of the statistical tables to employ. Let us assume, for example, that nine out of ten Englishmen are injured by residence in Madeira, but that nine out of ten consumptive persons are benefited by such a residence. These statistics, though imaginary, are possible and perfectly compatible. John Smith is a consumptive Englishman; are we to recommend a visit to Madeira in his case or not? In

other words, what inferences are we to draw about the probability of his death? Both of the statistical tables apply to his case, but they would lead us to directly contradictory conclusions. I do not mean, of course, contradictory precisely in the logical sense of that word, for one of these propositions does not assert that an event must happen and the other deny that it must; but contradictory in the sense that one would cause us in some considerable degree to believe what the other would cause us in some considerable degree to disbelieve. This refers, of course, to the individual events, the statistics are by supposition in no degree contradictory. Without further data therefore we can come to no decision.

Practically, of course, we should make our choice by the consideration that the state of a man's lungs has probably more to do with his health than the place of his birth has; that is, we should conclude that the duration of life of consumptive Englishmen corresponds much more closely with that of consumptive persons in general than with that of their healthy countrymen. But this is, of course, to import alien considerations into the question. The data, as they are given to us, and if we confine ourselves to them, leave us in absolute perplexity. It may be that the consumptive Englishmen almost all die when transported into the other climate; it may be that they almost all recover. If they die, this is in obvious

accordance with the first set of statistics; it will be found in accordance with the second set through the fact of the foreign consumptives profiting by the change of climate in more than their due proportion. A similar explanation will apply to the other alternative. The problem is therefore left absolutely indeterminate, for we cannot here appeal to any general rule so simple and so obviously applicable as that which, in a former case, recommended us always to prefer the more special statistics, when sufficiently extensive, to those which are wider and more general. We have no means here of knowing whether one is more special than the other; in fact such a term as special is inappropriate.

§ 16. And in this no difficulty can be found, so long as we confine ourselves to a just view of the subject. Let me again recall to the reader's mind what our present position is; we have substituted for knowledge of the individual (finding that unattainable) a knowledge of what occurs in the average of similar cases. But the conception of similarity in the cases introduces us to a perplexity; we manage indeed to evade it in many instances, but here it is inevitably forced upon our notice. There are here two aspects of this similarity, and they introduce us to two distinct averages. Two assertions are made as to what happens in the long run, and both of these assertions, by supposition, are verified. Of their truth there

need be no doubt, for both were obtained from experience.

§ 17. It may perhaps be supposed that such an example as this is a *reductio ad absurdum* of the principle upon which Life and other Insurances are founded. But a moment's consideration will show that this is quite a mistake, and that the principle of Assurance is just as applicable to examples of this kind as to any other. An Office need find no difficulty in the case supposed. They *might* (for a reason to be mentioned presently, they probably *would* not) insure the individual without inconsistency at a rate determined by either average. They might say to him, 'You are an Englishman. Out of the multitude of English who come to us nine in ten die if they go to Madeira. We will insure you at a rate assigned by these statistics, knowing that in the long run all will come right. You are also consumptive, it is true, and we do not know what proportion of the English are consumptive, nor what proportion of English consumptives die in Madeira. But this does not really matter for our purpose. The formula, nine in ten die, is in reality calculated upon these unknown proportions; for, being a statistical result, it must involve all such proportions. In other words, the unknown elements must, in regard to all the effects which they can produce, have been already taken into account. All the unknown conditions, therefore, will be found to

arrange themselves in accordance with the above statistical result. And this is sufficient for our purpose. But precisely the same language might be held to him if he presented himself as a consumptive man; that is to say, the Office could safely carry on its proceedings upon either alternative.

This would, of course, be a very imperfect state for the matter to be left in. The only rational plan would be to isolate the case of consumptive Englishmen, so as to make a separate calculation for their circumstances. This calculation would then at once supersede all other tables; for though, *in the end*, it could not arrogate to itself any superiority over the others, it would in the mean time be marked by fewer and slighter aberrations from the truth.

§ 18. The real reason why the Insurance office could not long work on the above terms is of a very different kind from that which some readers might contemplate, and belongs to a class of considerations which have been strangely neglected in the attempts to construct sciences of the different branches of human conduct. It is nothing else than that annoying contingency to which prophets since the time of Jonah have been subject, of uttering *suicidal* prophecies; of publishing conclusions which are perfectly certain when every condition and cause but one have been taken into account, that one being the effect of the prophecy itself upon those to whom it refers.

In our example above, the Office would get on very well until the consumptive persons found out what much better terms they could make by announcing their consumptiveness, and paying the premium appropriate to that class. But if they did this they would of course be disturbing the statistics. The tables were based upon the assumption that a certain fixed proportion (it does not matter what proportion) of the English lives would continue to be consumptive lives, which, under the supposed circumstances, would probably soon cease to be true. When it is said that nine Englishmen out of ten die in Madeira, it is meant that of those who come to the Office, as the phrase is, at random, or in their fair proportions, nine-tenths die. The consumptives are supposed to go there just like red-haired men, or poets, or any other special class. Or they might go in any proportions greater or less than those of other classes, so long as they adhered to the same practice throughout. The tables are then calculated on the continuance of this state of things; the practical contradiction is in supposing such a state of things to continue after the people had once had a look at the tables. If we merely make the assumption that the publication of these tables made no such alteration in the conduct of those to whom it referred, no hitch of this kind need occur.

§ 19. Examples subject to the ambiguity now

under consideration, will doubtless seem perplexing to the student unacquainted with the subject. They are, I think, quite irreconcileable with any other view of the science than that insisted on throughout this essay, viz. that we are only concerned with averages. It will perhaps be urged, there are two different values of the man's life in these cases, and they cannot both be true. Why not? The 'value' of his life is simply the number of years to which men in his circumstances do, on the average, attain; we have the man set before us under two different circumstances; what wonder, therefore, that these should offer different averages? In such an objection it is forgotten that we have had to substitute for the unattainable result about the individual, the really attainable result about a set of men as much like him as possible. The difficulty and apparent contradiction only arise when people will try to find some justification for their belief in the individual case. What can we possibly conclude, it may be asked, about this particular man John Smith? Nothing whatever, I reply; nor could we in reality draw a conclusion, be it remembered, in the former case, when we were practically confined to one set of statistics. There also we had what we called the 'value' of his life, and since we only knew of one such value, we came to regard it as in some sense appropriate to him as an individual. Here we have two values, belonging to different series, and as these

values are really different they seem discordant, but this complaint can only be made when we do what we have no right to do, viz. assign a value to the individual which shall admit of individual justification.

§ 20. Is it then perfectly arbitrary what series we select by which to judge? By no means; I have been trying to show throughout that in choosing a series we must seek for one the members of which shall resemble our individual in as many of his attributes as possible, subject only to the restriction that it must be a sufficiently extensive series. What I mean is, that in the above case, where we have two series, we cannot fairly call them contradictory; the only valid charge is one of incompleteness or insufficiency, a charge which applies in exactly the same sense, be it remembered, to all statistics which comprise genera unnecessarily wider than the species with which we are concerned. The only difference between the two different classes of cases is, that in the one instance we are on a path which we know will lead at the last, through many errors, to the truth (in the sense in which truth can be attained here), and we took it for want of a better. In the other instance we have two such paths, perfectly different paths, but either of which will lead us to the truth as before. Contradiction can only seem to arise when it is attempted to justify each separate step on our paths, as well as their ultimate conclusion.

We may now see why the Rule of Succession is ambiguous as well as erroneous, as described in the last chapter. When we observe a succession of individual things or events of any kind, the number of terms in the succession will depend upon the number of properties we take into account. And as it seems quite arbitrary how many of these properties we should take into account, the possible inferences we can draw will be various.

§ 21. The foregoing results may be thus summed up:—

Since the generalization of our statistics is found to belong to Induction, this process of generalization may be regarded as prior to, or at least independent of, Probability. We have, moreover, already discussed (in Chapter III.) the step corresponding to what are termed immediate inferences, and (in Chapter IV.) that corresponding to syllogistic inferences. Our present position therefore is that in which we may consider ourselves in possession of any number of generalizations, but wish to employ them so as to make inferences about a given individual; just as in one department of common logic we are engaged in finding middle terms to prove our argument. In this latter case the process is found to be extremely simple, no accumulation of different middle terms can lead to any real ambiguity or contradiction. In Probability, however, the case is different. Here, if we

attempt to draw inferences about the individual case before us, as often is attempted,—in the Rule of Succession for example,—we shall encounter the full force of this ambiguity and contradiction. Treat the question, however, fairly, and all difficulty disappears. Our inference really is not about the individuals as individuals, but about series or successions of them. We want to know whether John Smith will die within the year; this however cannot be known. But John Smith, by the possession of many attributes, belongs to many different series. The multiplicity of middle terms therefore is what ought to be expected. We *can* know whether a succession of men, paupers, consumptives, &c. die within a year. We may make our selection therefore amongst these, and in the long run the belief and consequent conduct of ourselves, and other persons (as described in Chapter III.), will be capable of justification. With regard to choosing one of these series rather than another, we have two opposing principles of guidance. On the one hand, the more special the series the better; for, though not more right in the end, we shall thus be more nearly right all along. But, on the other hand, if we try to make the series too special, we shall generally meet the practical objection arising from insufficient statistics.

§ 22. Throughout the discussions in this chapter it has been assumed that the common property which was observed in different individuals, and was then

by the Inductive act generalized throughout a definite or indefinite class, is one about the existence and amount of which there could never be any doubt or dispute. Most of the examples commonly given, and the discussions to which they often lead, tend very much to confirm such an opinion. I have deferred all examination of this point up to the present moment, in order not to break the line of enquiry; but it will be advisable now to devote some pages to ascertain how far the assumption spoken of above can be justified. The enquiry is, indeed, only indirectly connected with Probability, at least on the view of that science entertained in this Essay, but at the same time the connection though indirect is very close. Induction is involved in almost every example in Probability, and the nature of the relation between these sciences has been a source of so much error and confusion, that I have found it quite impossible to state accurately my own opinion about the latter science without making constant inroads into the former. It is the more necessary to state my opinion fully, in consequence of the great authority of Mr Mill being apparently in support of what I cannot but regard as an imperfect view of the subject.

§ 23. It will be necessary to commence with a definition of Induction. Let us start with that of Mr Mill, which, though given by him in various forms, will be expressed, I think, with substantial

accuracy as follows:—"Induction is that act of the mind by which, from a certain definite number of things or observations, we make an inference extending to an indefinite number of them." In this definition, which is in accordance with the investigation in the earlier part of this chapter, it is assumed that the data, viz. the limited number of things from which the formula starts, and on which it is grounded, are already clearly recognized. In every example, indeed, this recognition is almost necessarily presupposed before the example can be stated. Whether we take the simple symbolic one, this A and that A, and so on, are X; therefore every A is X; or any special concrete instance that the Inductive Sciences may offer, all practical difficulty which may have existed as to discovering and recognizing our A is omitted from view. But though this omission is possible in examples, it is scarcely possible in making original inferences. The objects from which our inference started as its basis must have been selected; and since this selection was neither made at random nor performed for us by others, there must have been some principle of selection in our minds. The selection may appear little more than a guess sometimes, but even then it is the sagacious guess of one whose mind has been disciplined to the work.

§ 24. As most readers of these pages will know, Dr Whewell has strongly insisted on this selective or,

as it may almost be termed, *creative* part of the process of Induction, and has applied a particular technical expression to denote it. Being probably one of the few who have deduced their philosophical schemes from a laborious investigation of the processes by which science has been actually constructed, he has not unnaturally attached extreme importance to this part of the act of Induction, and has incorporated it into his definition of Induction. He sometimes uses rather strong expressions to describe it, but I think that what I have just mentioned is all that is meant when he speaks of the element which is introduced by the mind, and is not found in the things. The data for the Induction, therefore, have to be selected by a process which the common examples tend to let drop out of sight.

Let us take one of these examples for closer examination, for instance, the familiar one:—This man is mortal, that man is mortal, and so on; therefore all men are mortal. Now here it is obvious that the 'conception,' to use Dr Whewell's expression, is one with which we are already familiar. The collection of attributes which make up what we understand by humanity has been so constantly associated together, and this association has been so universally aided by common language, that nobody can see one of the objects which contain these attributes without having the class recalled to him, or at least without having

some of the objects which compose the class clearly separated off and brought before his mind. And similarly with regard to the attribute of mortality. The words 'man' and 'mortal' suggest to every mind the appropriate conceptions. Now though, as we have already seen, and as will be noticed again soon, there is still room even here for much ambiguity, this familiarity with the conceptions enormously diminishes the difficulty of making inferences. It completely alters the character of that process in fact; so that instead of being like a drive to the right point over the open plain, it now resembles the choice of one out of a limited number of ruts.

§ 25. A slight modification of the example will make it wear a very different aspect. Let us try to find a case in which the conception is not marked out already for us by a word, and we shall see what a different relative importance is then assumed by the two parts of the Inductive process. There is some difficulty, it must be observed, in finding an example of the kind required, for unless there be a word already applied to the objects which we group together the example will become very awkward to state. But we may obviate the difficulty as follows. Let us take an example in which we, having the word, are already familiar with the conception, but suppose the inference to be made by people who have not the word, the person who makes the inference will

then labour under the difficulty mentioned, whilst we who stand by and criticise him will escape it.

Let us take, then, the following example. This consumptive man, and that consumptive man, and so on, are short-lived; therefore, generally, all consumptive persons are short-lived. To us this example is as simple as the last, but suppose the inference to be made for the first time by some one amongst a people where consumption had not yet been sufficiently known to be marked out and named. The process will then, *to them*, assume a very different form. The cough, the shortness of breathing, &c., which the word at once suggests to us, will have to be singled out and distinguished from a multitude of somewhat similar phenomena. There is room here for an almost infinite amount of observation and experiment. But now suppose that any one had got to this point, and, what is more, had connected the qualities of consumption and short-life in two or three instances. By this time he has gone partly through the process of Induction according to Dr Whewell, but he has only reached its threshold according to Mr Mill. But what more is there left for him to infer? Surely every one who had got to this point would go on to infer, therefore all consumptive persons die soon. Whether they would be right or wrong in so doing is of course immaterial to our present purpose. The inference would be made, and

therefore what Mr Mill considers to be Induction would take place, so simply and certainly as to be performed almost unconsciously.

§ 26. But there is still another source of doubt and confusion. It needs but a slight observation to perceive that even in the former example, that of man's mortality, the inference is not so simple as it is made to appear. With all the immense help of the previous inductions and observations of others, which are fixed and perpetuated for us in language, there is still a degree of arbitrariness in the process as it is given in common examples. If we try to place ourselves in the position of one making the inference for the first time we shall see that we might then be involved in serious perplexity. It should be borne in mind that when we state the grounds of the inference in the form, this man is mortal, and that man is; we are presupposing that the observer has already not merely distinguished the class 'man,' but distinguished it as appropriate to his immediate purpose. Whereas the grounds of his inference were in reality certain *objects*. It is true that these objects belong to the class man, but, as I have pointed out, they belong also to an indefinite number of other classes as well; to classes within that of man; for example, Greek, European, &c.: to classes without that of man; for example, mammalia, animal, organized being. As it happens, the observer would have been right in his

inference whichever of these classes and corresponding conceptions he might have selected; but such considerations show us that there was scope for great ingenuity and for considerable effort of mind in a preliminary process, which, according to Mr Mill, is no part of Induction.

When therefore Mr Mill states, as he does in another part of his volume, that the mortality of John, Thomas, &c. is the *whole* evidence for the death of Socrates, and for that of men in general, he is, as it appears to me, very much underrating the complexity of the problem. These objects belonged, no doubt, to the class *man*, but they belonged to an indefinite number of other classes also. What made us draw the line just where we did? Why did we not go farther? Is not our belief strengthened by the death of animals? Is it not influenced by that of organized beings generally? The moment we admit the question to be so indefinite as this, we see that so far from the arbitrary selection of instances, which was given to us, being the whole evidence, it is but a fragment of the evidence. Analogies of every conceivable amount of strength press in upon us from every side, and help to swell or diminish the degree of our belief in the final result.

§ 27. The bearing of all this upon rules of discovery is obvious. In the precise form in which such rules are commonly stated in Probability it is

supposed that when an event has been observed to happen in a certain way, or an object to possess a certain attribute, a given number of times in succession, we are able to assign the degree of belief which should be entertained as to the recurrence of this event or property. The rule, in this particular form, was fully examined and shown to be false, but we are now able to perceive that independently of its falsity any rule of the kind is impracticable. It is not merely that we are forbidden to say that, because three A's have been found to be X it is therefore 4 to 1 that the next A will be X. Before the problem could be *stated* even, a process that is often very difficult and tedious has to be gone through. A and X have to be recognized and distinguished, and if the conception is one with which we are not familiar this distinction will be very partial and progressive. And when the conception is formed, we should generally find it impossible to limit our grounds of belief to the objects denoted by this conception; an indefinite number of other conceptions would all seem to have an almost equal claim to acceptance. It would be quite arbitrary to reject entirely all these other conceptions, and if we do not reject them the data on which our belief is founded become almost infinite in number, and therefore vague in value. Things and qualities, it must be remembered, are far from being so sharply discriminated

from one another as are letters and symbols. We have observed consumptiveness in Englishmen; is it quite the same quality in people of other countries? Is there not something resembling it in some animals? We may give the quality the same name, but in doing so we must remember that it possesses every conceivable amount of variation in degree; and if we are reasoning with any accuracy this variation should influence the amount of our assent. Moreover, in addition to the indefiniteness caused by the object being thus included in a number of classes which point with varying degrees of force towards the same inferences, there is (as already pointed out) the embarrassment caused by its being included in classes which point towards conflicting inferences. I do not see then how such rules of Anticipation can ever be more than a collection of hints and suggestions for making judicious guesses. When any one has obtained the conception of consumption, and has observed that it is accompanied in certain known cases by short-life, he may and probably will go on to the inference that it will be so in more cases, if not in all. The proposition, therefore, All consumptive persons die early, is gradually borne in upon him. He cannot point to any limited, definite number of instances on which this Induction rests; it is supported rather by an indefinite number of analogies more or less close or remote. But the first point at which

anything like scientific reasoning can commence is the point at which this proposition is obtained.

Precisely similar is the difficulty, and equally gradual the process, of obtaining any one of the proportional propositions with which we have been concerned. But when such propositions are obtained we are then in a position to draw certain inferences by strict rules. If they are universal propositions they belong to ordinary Logic, if proportional to Probability.

CHAPTER VIII.

ON DIRECT AND INVERSE PROBABILITY.

§ 1. We must now take some notice of a distinction, commonly drawn in works on this subject, between what is called Direct and Inverse Probability. The distinction is thus introduced by De Morgan*: "In the preceding chapter we have calculated the chances of an event, knowing the circumstances under which it is to happen or fail. We are now to place ourselves in an inverted position: we know the event, and ask what is the probability which results from the event in favour of any set of circumstances under which the same might have happened."

On the principles of the science involved in the definition which was discussed and adopted in the earlier chapters of this work, the reader will easily infer that no such distinction as this can be regarded as fundamental. One common feature was traced in all the subjects which were to be referred to Probability, and from this feature the possible rules of inference were immediately derived. All other distinctions are merely of arrangement or management.

* *Essay on Probabilities*, p. 53.

The apparent importance of the distinction under discussion arises entirely, I cannot but think, from the attempt to force the Calculus of Probability upon a class of subjects which do not properly belong to it, and about which an arbitrary supposition must be made before the rules admit of application.

§ 2. This will be best seen by the examination of special examples; as any, however simple, will serve our purpose, let us take the two following:—

(1) A ball is drawn from a bag containing nine black balls and one white; what is the chance of its being the white ball?

(2) A ball is drawn from a bag containing ten balls, and is found to be white; what is the chance of there having been but that one white ball in the bag?

The class of which the first example is a simple instance has been already abundantly discussed. The interpretation of it is as follows: If balls be continually drawn and replaced, the proportion of white ones to the whole number drawn will tend towards the fraction $\frac{1}{10}$. The contemplated action is a single one, but we view it as one of the above series; at least our opinion is formed upon that assumption. We conclude that we are going to take one of a series of events which may appear individually fortuitous, but in which in the long run those of a given kind are one-tenth of the whole; this kind (white) is then

singled out by anticipation. By stating that its chance is $\frac{1}{10}$, we merely mean to assert this physical fact, together with such other mental facts, impressions, inferences, &c., as may be properly associated with it.

§ 3. Have we to interpret the second example in a different way? Here also we have a single instance, but the nature of the question would seem to decide that the only series to which it can properly be referred is the following :—Balls are continually drawn from *different* bags each containing ten, and are always found to be white; what is ultimately the proportion of cases in which they will be found to have been taken from bags with only one white ball in them? Now it was shown in the last chapter that time has nothing to do with the question; omitting therefore the consideration of this element, we have for the two series from which our opinions in these two examples respectively are to be formed :—(1) balls of different colours presented to us in a given ratio; (2) bags with different contents similarly presented. From these data we have to assign their respective weight to our anticipations of (1) a white ball; (2) a bag containing but one white ball. So stated the problems would appear to be formally identical, the only difference being in the matter. Theory therefore would recognize no distinction between them.

When, however, we begin the practical work of solving them we perceive a most important distinction. In the first example there is not much that is arbitrary; balls would under such circumstance really come out in somewhat the proportion expected. Moreover, it does not seem an unfair demand to say that the balls are to be 'well-mixed' or 'fairly distributed,' or to introduce any of the other conditions by which, under the semblance of judging *à priori*, we take care to secure our prospect of a series of the desired kind. But we cannot say the same in the case of the second example.

§ 4. The line of proof by which it is generally attempted to solve the second example is of this kind; —It is shewn that there being one white ball for certain in the bag, the only possible antecedents are of ten kinds, viz. bags, each of which contains ten balls, but in which the white balls range respectively from one to ten in number. This of course limits the kind of terms to be found in our series. But we want more than this limitation; we must know the proportions in which these terms are ultimately found to arrange themselves in the series. Now this requires an experience about bags which is not given to us. If therefore we are to solve the question at all we must make an assumption; let us make the following;—*that each of the bags described above occurs equally often*,—and see what follows. The bags being

drawn from equally often, it does not follow that they will yield equal numbers of white balls. On the contrary they will, as in the last example, yield them in direct proportion to the number of such balls which they contain. The bag with one white and nine black will yield a white ball once in ten times; that with two white, twice; and so on. The result of this, it will be easily seen, is that in 100 drawings there will be obtained on the average 55 white balls. Now with those drawings that do not yield white balls we have, by the question, nothing to do, for that question, as it was originally stated, postulated the drawing of a white ball. The series we want is therefore composed of those which do yield white. Now what is the additional attribute which is found in some member of this series, and which we mentally anticipate? Clearly it is the attribute of having been drawn from a bag which only contained one of these white balls. Of these there is out of the 55 drawings but one. Accordingly the required chance is $\frac{1}{55}$. That is to say, that the white ball will have been drawn from the bag containing only that one white, once in 55 times.

§ 5. Now, with the exception of the passage in italics, the process here is precisely the same as in the other example; it is somewhat longer only because we are not able to appeal immediately to experience, but are forced to try and deduce what the

result will be, though the validity of this deduction itself rests, of course, upon experience. But the above passage is a very important one. It is scarcely necessary to point out how entirely arbitrary it is. The nature of the assumption is commonly disguised by saying that, where we have no reason to expect one kind of bag rather than another it is only reasonable to regard all possible kinds as equally likely. But, as I have before insisted, this phrase 'equally likely' is one that must submit itself to explanation. If any one replies that he means nothing more by it than that he expects the events equally, he is at liberty to do so; but he should remember that he is applying the words entirely to his state of mind, and avowing that they have no necessary connection with experience. If they are to have such a connection he can only mean that the events do happen equally often; for our opinion about the events being equally likely is simply worthless, indeed should rather be called a guess, unless we have good reason to believe that it is really in harmony with experience. Now would it not somewhat surprise an unprejudiced person if his assent were demanded to the proposition that, in his experience in this world, bags of the kind described above occurred with equal frequency? or even if he had merely to assert that it was his honest conviction that they would do so? We may assume, as in the common hypothesis, that

such are the conditions under which the course of nature is carried on; but it is only fair that the assumption should be openly recognized.

§ 6. I will now take an instance which shall be entirely from nature, so as not to require an arbitrary supposition of the kind just discussed; it will then be seen that this distinction between Direct and Inverse Probability disappears altogether, or resolves itself into one of *time* merely, which, as was shown in the last chapter, is entirely alien to our subject.

(1) What is the chance that if A. B. dies he will die of typhus fever?

(2) A. B. is dead; what is the chance that he died of typhus?

If we refer to the statement of De Morgan at the commencement of this chapter, we shall see that these two examples undoubtedly fall under the head of what he there defines as Direct and Inverse Probability. If therefore the distinction breaks down in this case it cannot be an essential distinction, for this cannot be affected by the simplicity of the example chosen. Now a moment's consideration will show that, considered as questions in Probability, these two examples are absolutely identical. In each alike the enquiry is, What weight should be attached, prior to examination of the case, to the anticipation that a man's death is caused by typhus fever? The statistical data by which this question is to be an-

swered, of course, are also the same, viz. Of the total number of deaths, what is the proportion caused by that disease? There is to be sure this distinction, which may be important in other respects, that in the first example what is an anticipation to us is also an anticipation to all other people, owing to the fact of the man being yet alive; whereas in the second example, the man being now dead, other people know already, and we might probably if we took the trouble, ascertain, what the cause of death really was. But when these questions are treated as examples in Probability, it would be wandering from the point to found a distinction upon the difference that would exist between them if they were not so treated.

§ 7. Considered, therefore, as a portion of the theory of the subject, the distinction between Direct and Inverse Probability must be abandoned; but the discussion of it may serve to direct renewed attention to another and far more important distinction. It will remind us that there is one class of examples to which the calculus of Probability is rightfully applied, because statistical data are all we have to judge by; whereas there are other examples in regard to which, if we will insist upon making use of these rules, we may be deliberately abandoning the opportunity of getting far more trustworthy information by other means. This is a point to which reference will be made again in a future chapter.

It will show too, with respect to some examples of the latter kind, how much there is that is wholly arbitrary in the ordinary treatment of the subject. Writers will apply their rules to instances in which not merely other evidence *is* at hand, but in which statistics are *not*. There being positively no experience of any kind upon the subject, either we have to divorce the science from experience and make it therefore the study of our own unauthorized impressions, or else we find ourselves at a loss. But as it is a hard thing to be deprived of the right of forming a positive opinion merely because we happen to have no grounds on which to rest it, we are driven to assume for ourselves a fictitious experience, as already described, which opens the way at once to confusion and dispute.

CHAPTER IX.

CRITICISM OF SOME COMMON CONCLUSIONS IN PROBABILITY.

§ 1. The view taken in the seventh chapter of the connection between Probability and Induction will suggest considerable doubts as to the validity of some commonly accepted conclusions of the former science. There is, for instance, a difficulty frequently anticipated and discussed by writers upon the subject. They assure those who are beginning their studies in it that no anterior improbability is a bar to a thing happening in time; in fact, if there *be* only an improbability, and not an impossibility, this insures that the thing shall happen. In other words, whatever be the odds against an event, if we only go on long enough the event is sure to come to pass at length. That three pence should all give heads is not so likely as that one should do so; that four should give heads is still more unlikely, and so on; but all these events occur in their due time. Proceeding in this way it is inferred that ten heads from as many pence, though, of course, very unlikely, will yet be found in its due proportion of instances; viz. once in 1024 times. It

is assumed that such an inference will be received with some doubt, and arguments are given to convince the hesitating student of its truth. He is reminded that improbability, however extreme, is not only different from impossibility, but positively excludes it.

As an explanation of part of the difficulty, the above remarks are very useful and to the point. There are many persons to whom a good deal of illustration will be necessary before they can realize that to say that the chances are enormously against a thing is only another way of saying that the thing will really happen occasionally though very rarely. I confess however that this answer appears to me to leave the main difficulty untouched, by assuming the real questions at issue. If the chances be 1023 to 1 against the above-mentioned occurrence, it undoubtedly will happen once in 1024 times. This is the meaning of the statement; it is in fact but another form of expression. But *are* the chances 1023 to 1 against the event? This is the point upon which we are now going to give a brief discussion.

§ 2. To the question, when stated in this form, the very summary answer may perhaps be given, that to doubt the result is to doubt the truth of mathematics, with the proviso, of course, that we are talking of an ideal penny. It has been stated already that a penny may be idealized, but that to talk of performing this process upon 'randomness' and the

other conditions which are all equally coefficients of the cause by which the effect is produced, is to use words with little meaning; at least the meaning can only be that the effect, *i.e.* in this case the succession of throws, is apportioned in the way supposed to be assigned by the chance, so that we are in a circuitous way talking about the succession itself. It is of no use therefore to speak of ideal probabilities; we must adopt one or other of these alternatives;—either we are working out a purely arithmetical sum of combinations and permutations, choosing to apply to the results the names of 'chances;' or we are making inferences about the actual behaviour of objective things, in which case though our results may fail of perfect accuracy, they must at least have a fair foundation of fact. If the former alternative be adopted, I have nothing more to say, as the present is not a treatise upon any branch of mathematics. But if the latter be adopted, the question may be fairly asked, Does the formula give accurate results?

§ 3. We will give the rule the considerable advantage of assuming that the pence are perfect, so that in the long run they show no preference for either head or tail; the question then remains, Will the repetitions of the same face obtain the proportional shares to which they are entitled, if the theory be correct? We intend to refer to high numbers, but

the illustration will be simpler if we begin with a small one, for example, a repetition of two. Putting then, as before, for the sake of brevity, H for head, and HH for heads twice running, we are brought to this issue;—Given that the chance of H is $\frac{1}{2}$, does it follow necessarily that the chance of HH (with two pence) is $\frac{1}{4}$? To say nothing of 'H ten times' occurring once in 1024 times (with ten pence), need it occur at all? The mathematicians, for the most part, seem to think that this conclusion follows necessarily from first principles; to me it seems to rest upon no more certain evidence than a reasonable extension by Induction.

§ 4. Taking then the possible results which can be obtained from a pair of pence, what do we find? Four different results may follow, namely, (1) HT. (2) HH. (3) TH. (4) TT. If it can be proved that these four are equally probable, that is, occur equally often, the commonly accepted conclusions will follow, for a precisely similar argument would apply to all the larger numbers.

The proof usually advanced makes use of what is called the Principle of Sufficient Reason. It takes this form;—Here are four kinds of throws which may happen; once admit that the separate elements of them, namely, H and T, happen equally often, and it will follow that the above combinations will also happen equally often, for no reason can be given in

favour of one of them that would not equally hold in favour of the others.

To this mode of argument reference has already been made more than once. Surely some stronger reasons ought to be given for believing in a result than an inability to see why it should not come as soon as anything else. But, even if the rule were admitted to be valid, I think it is far too readily assumed that the rule would be applicable to prove the result in question. Let us examine the proof somewhat more closely. There are four different results which may happen. In the symbolic representation of them given above they appear to differ from one another, for the letter H is different from the letter T; but this difference is of course entirely owing to our notation, the sides themselves of the pence having been expressly idealized into absolute similarity*. As between the single faces therefore H and T, the rule if sound would be applicable; no

* I am endeavouring to treat this rule of Sufficient Reason in a way that shall be legitimate in the opinion of those who accept it, but there seem very great doubts whether a contradiction is not involved when we attempt to extract results from it. If the sides are absolutely alike, how can there be any difference between the terms of the series? The succession seems then reduced to a dull uniformity, a mere iteration of the same thing many times; the series we contemplated has disappeared. If the sides are not absolutely alike, what becomes of the applicability of the rule?

distinction can be observed between these except the merely apparent one arising out of our own notation. To suppose H therefore to occur more often than T, namely head to occur more often than tail, would be an infraction of the rule. But it seems to be too hastily assumed that the same must be the case as between the *pairs* of faces already mentioned.

To a certain extent I admit the validity of the rule for the purpose. In the series given above it would be valid to prove the equal frequency of (1) and (3) and also of (2) and (4); for there is no difference existing between these pairs except what is introduced by our own notation. TH is the same as HT, except in the order of the occurrence of the symbols H and T, which we do not take into account. But either of the pair (1) and (3) *is* different from either of the pair (2) and (4). Transpose the notation and there would still remain here a distinction which the mind can recognize. A succession of the same thing twice running is distinguished from the conjunction of two different things by a distinction which does not depend upon our arbitrary notation only, and would remain entirely unaltered by a change in this notation. The principle therefore of Sufficient Reason, if admitted, would only prove that doublets of the two kinds, for example, (2) and (4), occur equally often, but it would not

prove that they must each occur once in four times. It cannot be proved indeed in this way that they need ever occur at all.

§ 5. The formula, then, not being demonstrable *à priori*, (as might have been concluded,) can it be obtained by experience? To a certain extent it can; the present experience of mankind in pence and dice seems to show that the smaller successions of throws do really occur in about the proportions assigned by the theory. But how nearly they do so, no one can say, for the amount of time and trouble to be expended before we could feel that we have verified the fact, even for small numbers, is very great, whilst for large numbers it would be simply intolerable. The experiment of throwing often enough to obtain 'heads ten times' has been actually performed by two or three persons, and the results are given by De Morgan. This, however, being only sufficient on the average to give 'heads ten times' a single chance, the evidence is very slight; it would take a considerable number of such experiments to set the matter at rest.

Any such rule, then, as that which we have just been discussing, which professes to describe what will take place in a long succession of throws, is only conclusively proved by experience within very narrow limits, that is, for small repetitions of the same face; within limits less narrow, indeed, we feel assured

that the rule cannot be flagrantly in error, otherwise the variation would be almost sure to be detected. From this we feel strongly inclined to infer that the same law will hold throughout. In other words, we are inclined to extend the rule by Induction and Analogy. Still there are so many instances in nature of laws which hold within narrow limits but get egregiously astray when we attempt to push them to great lengths, that we must give at best but a qualified assent to the truth of the formula. I breathe no suspicion, let it be observed, against the integrity of the mathematics introduced, but only deny that they can be taken as authoritative about the physical facts to which they are applied. In other words, we cannot be sure that we have obtained the right mathematical formula.

§ 6. The object of the above reasoning is simply to show that we cannot be certain that the rule is true. Let us now turn for a minute to consider the causes by which the succession of heads and tails is produced, and we may perhaps see reasons to make us still more doubtful.

It was shown in Chapter II. that in calculating probabilities *d priori*, as it is called, we were only able to do so by introducing restrictions and suppositions which were in reality equivalent to assuming the expected results. We used words which in strictness mean, Let a given process be performed; but

an analysis of our language, and an examination of various tacit suppositions which made themselves felt the moment they were not complied with, soon showed that our real meaning was, Let a series of a given kind be obtained; it is to this series only, and not to the conditions of its production, that all our subsequent calculations properly apply. In the present instance this transformation from an actual and limited to an ideal and unlimited series cannot be allowed, for our express object is to examine into the validity of these suppositions. The physical process being performed, we want to know whether anything resembling the contemplated series really will be obtained.

Now if the penny were invariably set the same side uppermost, and thrown with the same velocity of rotation and to the same height, &c.—in a word, subjected to the same conditions,—it would always come down with the same side uppermost. Practically, we know nothing of this kind occurs, for the individual variations in the results of the throws are endless. Still there will be an *average* of these conditions, about which the throws will be found, as it were, to cluster much more thickly than elsewhere. We should be inclined therefore to infer that if the same side were always set uppermost there would really be a disturbance in the series which we ordinarily look for. In a very large number of throws we should probably begin to find, under such

circumstances, that either head or tail was having a preference shown to it. If so, would not similar effects be found to be connected with the way in which we started each successive *pair* of throws? According as we chose to make a practice of putting HH or TT uppermost, might there not be a disturbance in the proportion of successions of two heads or two tails? Following out this train of reasoning it would seem to point with some likelihood to the conclusion that in order to obtain a series of the kind we expect, we should have to dispose the antecedents in a similar series at the start. The changes and chances produced by the act of throwing might introduce infinite individual variations, and yet there might be found, in the very long run, to be a close similarity between these two series.

This is, to a certain extent, only shifting the difficulty, I admit; for the claim formerly advanced about the possibility of proving the proportions of the throws in the former series, will probably now be repeated in favour of those in the latter. Still the question is very much narrowed, for we have reduced it to a series of *voluntary* acts; a man may put whatever side he pleases uppermost. He may act consciously, as I have said, or he may think nothing whatever about the matter, that is, throw at random; if so it will probably be asserted by many that he will involuntarily produce a series of the kind in

question. It may be so, or it may not; it does not seem that there are any easily accessible data by which to decide. All that I am concerned with here is to show the likelihood that the commonly received result does in reality depend upon the fulfilment of a certain condition at the outset, a condition which it is certainly optional with any one to fulfil or not as he pleases. The small numbers doubtless will take care of themselves, owing to the infinite complications produced by the casual variations in throwing; but the large ones may suffer, unless their interest be consciously or unconsciously regarded at the outset.

An illustration may serve at once to explain and give support to the above view. Suppose that on a chess-board a number of distinct heaps of sand are arranged. Let the board be sharply struck from beneath; the grains will fly up and then settle down again; their arrangement being considerably disturbed by the process. But still there will be distinguishable traces of their former distribution; unless there were a good many large heaps before, we shall not find them afterwards. Single grains will be scattered all about, but the clusters will be less disturbed either in position or relative magnitude; and thus the former arrangement will really be found again on the whole, though disturbed and modified. Something of this disturbance, and no more, may be pro-

duced in the tossing up of a penny. In a very large number of throws we may find the order at the outset reproduced with infinite individual variations; and thus the long successions may not turn up in the end, at least not in their right proportion, unless we ourselves put them there in the beginning.

§ 7. The advice 'only try long enough, and you will sooner or later get any result that is possible,' is plausible, but it rests only on Induction and Analogy; mathematics do not prove it. As has been so often stated, there are two distinct views of the subject. Either we may, on the one hand, take a series of symbols, call them heads and tails; HT; &c.; and make the assumption that each of these, and each pair of them, and so on, recurs in the long run with a regulated degree of frequency. (All these, it is to be observed, being perfectly distinct assumptions.) We may then calculate their combinations and permutations, and the consequences that may be drawn from the data assumed. This is a purely algebraical process; it is infallible; and there is no limit whatever to the extent to which it may be carried.

This view may be, and undoubtedly should be, nothing more than the expression of what I have called the substituted or idealized series which generally has to be introduced as the basis of our calculation. The danger to be guarded against is that

of regarding it too purely as an algebraical conception, and thence of sinking into the very probable errors both of too readily evolving it out of our own consciousness, and too freely pushing it to unwarranted lengths.

Or on the other hand, we may consider that we are treating of the behaviour of *things*;—balls, dice, births, deaths, &c.; and drawing inferences about them. But, then, what were in the former instance allowable assumptions, become here propositions to be tested by experience. Now the whole theory of Probability as a practical science, in fact as anything more than an algebraical truth, depends of course upon there being a close correspondence between these two views of the subject, in other words, upon our substituted series being kept in accordance with the actual series. Experience abundantly proves that, between considerable limits, in the example in question, there does exist such a correspondence. But let no one attempt to enforce our assent to every remote deduction that mathematicians can draw from their formulæ. When this is attempted the distinction just traced becomes prominent and important, and we have to choose our side. Either we go over to the mathematics, and so lose all right of discussion about the things; or else we take part with the things, and so defy the mathematics. We do not question the formal accuracy of the latter within

their own province, but either we dismiss them as irrelevant, as applying to data of whose correctness we cannot be certain, or we take the liberty of remodelling them so as to bring them into accordance with facts.

§ 8. The extreme importance of obtaining a clear apprehension of the above distinction, has induced me to devote what might seem needless trouble to the illustration of it. A single example will serve to show the conclusions to which some thinkers have been led by consistently working out the data they have adopted. M. Quetelet, in his work on Probabilities, has discussed* "*the determination of the law of occurrence of two kinds of events, the chances of which are perfectly equal*, and which may happen either separately or simultaneously, but in different combinations." The first half of this sentence is perfectly plain; it means that the two kinds of events do, on the average, occur equally often. The latter part however is somewhat obscure; it appears to assume nothing, talking only about the way in which the events *may* happen. As would easily be seen, however, on examination, it introduces a very definite supposition as to how they *will* happen; namely that, in accordance with the assumption criticized in this chapter, all the different combinations of the same number will occur equally

* Quetelet *On Probabilities*, by O. G. Downes, p. 61. The italics are my own.

often. He selects the example of births and deaths as found succeeding one another in a register. He assumes very justly (the number of males and females being equal) that the chance of any one entry being male is one half. Then follows the next step, that the chance of having two males succeeding is one fourth. I have endeavoured to show that this is a distinct supposition, which cannot certainly be deduced from the phrase in italics. By following out the above process, the conclusion is arrived at that once in a certain determinate number of times we shall find the deaths of ten males happening successively. Thinking it possible that one might like to know "how far experience justifies the calculation," and being a humourist as well as a mathematician, he remarks that the process of actually consulting the registers themselves would be "tedious," and that he will therefore resort to "experiments more expeditious and quite as conclusive." He therefore puts forty black and forty white balls into a bag, proceeds to draw them, and to note the successions of each colour that come out, and this is supposed to prove that men and women will die in certain proportions. If by men and women be meant black and white balls, I have no objections to offer; but if the words denote anything more, one might be inclined to demur to some of his conclusions. I am quite aware that any hesitation to accept these conclusions would be met by the enquiry, whether it is

doubted that the events in question are independent, and their individual occurrence equally probable. This word 'independence' has already been discussed; under an appearance of specious modesty it really makes very extensive claims; I can only say therefore that in the sense which must be assigned to the word, to justify the consequences which are commonly supposed to follow from it, it could hardly be proved that the events are independent. Under ordinary circumstances no perceptible deflection from the theory might be observed, but on the rare occasions on which any large number of one sex did happen to die in succession it is quite possible that this might introduce a disturbance amongst the proportions of the deaths which succeeded such an occurrence.

CHAPTER X.

THE APPLICATION OF PROBABILITY TO TESTIMONY.

§ 1. On the principles which have been adopted and adhered to in this work, it will easily be seen that several classes of problems will have to be excluded from the science of Probability which may seem to have acquired a prescriptive right to admission. The most important, perhaps, of these refer to what is commonly called the credibility of testimony. Almost every treatise upon the science contains a discussion of the principles according to which credit is to be attached to combinations of the reports of witnesses of various degrees of trustworthiness, or the verdicts of juries. A great modern mathematician, Poisson, has written an elaborate treatise expressly upon this subject; whilst a considerable portion of the works of Laplace, De Morgan, Quetelet, and others, is devoted to an examination of similar enquiries. It would be presumptuous to differ from such authorities as these, except upon the strongest grounds; but I confess that the extraordinary ingenuity, research, and mathematical ability which have been devoted to these problems, considered as questions in Probability, fail to convince

me that they ever ought to have been so considered. I proceed to give the grounds for this opinion.

§ 2. It will be remembered that in the course of the chapter on Induction we entered into a detailed investigation of the process required when, instead of the appropriate series from which the deduction was to be made being set before us, the *individual* presented himself and the task was imposed upon us of selecting the requisite series. At such a stage we may of course assume that the preliminary process of obtaining the statistics which are extended into these series has been already performed; we may suppose therefore that we are already in possession of a quantity of series, our only doubt being as to which of them we should then employ. This selection was shewn to be to a certain extent arbitrary; for, owing to the fact of the individual possessing a large number of different properties, he became in consequence a member of different series, which might present different averages. We must now examine somewhat more fully than we did before the practical conditions under which any difficulty arising from this source ceases to be of importance.

§ 3. One condition of this kind is very simple and obvious. It is that the different statistics with which we are presented should not in reality offer materially different results. If, for instance, we were enquiring into the probability of a man aged forty dying within

the year, we might if we pleased take into account the fact of his having red hair, or being born in a certain county or village. Each of these circumstances would serve to specialize the individual, and therefore to restrict the limits of the statistics which were applicable to his case. But the consideration of such qualities as these would either leave the average precisely as it was, or produce such an unimportant alteration in it as no one would think of taking into account.

Or again; although the different sets of statistics may not as above give almost identical results, yet they may do what practically comes to very much the same thing, that is, arrange themselves into a small number of groups, all of the statistics in a group coinciding in their results. If for example a consumptive man desired to insure his life, there would be a marked difference in the statistics according as we took his peculiar state of health into account or not. We should here have two sets of statistics, natural kinds they might almost be called, which would offer decidedly different results. If we were to specialize still further, by taking into account insignificant qualities like those mentioned in the last paragraph, we might indeed get more limited sets of statistics applicable to persons more closely resembling the individual in question, but these would not differ sufficiently in their results to make it worth our while to do so. In other words, the different series which

are applicable arrange themselves into a limited number of groups, whence the range of choice amongst them is very much diminished in practice.

§ 4. It may serve to make the foregoing remarks clearer to express them under a slightly different form. It was shewn in the first two chapters that, in the enquiries to which Probability introduces us, we are concerned with a series or indefinitely extensive class which is fixed by the presence of permanent attributes, the individuals of it being differenced (and thence a sub-class created) by the presence or absence of certain variable attributes. The conditions mentioned above are equivalent to asserting that these classes must be easily distinguishable; in other words, since the class is distinguished by means of certain attributes, these attributes must either be confined to it, or, if they are found elsewhere, must exist there in easily distinguishable degrees or in different combinations.

§ 5. The reasons for the conditions above described are not difficult to detect. Where these conditions exist the process of selecting a series or class to which to refer any individual is very simple, and the selection is final. The process is simple, for there being but a few classes, and these defined by easily distinguishable attributes, all we have to do in any particular case is to ascertain whether these attributes exist, which ought not in general to offer any difficulty. The selection also is final; for though the individual possesses many

other attributes which, or the statistics appropriate to which, we may gradually come to recognize, these will not affect the result to any appreciable degree. It is assumed, as above described, that the consideration of these minor qualities does not materially disturb the statistics. We do not therefore trouble ourselves about their existence when we have once determined the statistics by which we mean to judge. In any case of insurance, for example, the question we have to decide is of the very simple kind; Is *A. B.* a man of a certain age? If so one in ten like him die in the course of the year. If any further questions have to be decided they would be of the following description. Is *A. B.* a healthy man? Does he follow a dangerous trade? But here too the classes in question are but few, and the limits by which they are bounded are tolerably precise; hence the reference of an individual to them is easy. And when we have once chosen our class we remain untroubled by any further considerations; for since no other statistics are supposed to offer a materially different average, we have no occasion to take account of any other properties than those already noticed.

§ 6. Let us now examine how far the above conditions are fulfilled in the case of problems which discuss what is called the credibility of testimony. The following would be a fair specimen of one of the elementary enquiries out of which these problems are

composed;—Here is a statement made by a witness who lies once in ten times, what am I to conclude about its truth? Objections might fairly be raised against the possibility of thus assigning a man his place upon a graduated scale of mendacity. This however we will pass over, and will assume that the witness goes about the world bearing stamped on his face the degree of credit to which he has a claim. But there are other and even stronger reasons against the admissibility of this class of problems.

§ 7. That which has been described in the previous sections as the 'individual' which had to be assigned to an appropriate class or series of statistics is, of course, in this case, *a statement*. In the particular instance in question the individual is already assigned to a class, that namely of statements made by a witness of a given degree of veracity, but it is clearly optional with us to confine our attention to this class in forming our judgment; at least it would be optional to do this whenever we were practically called on to form such an opinion. But in the case of this statement, as in that of the man whose insurance we were discussing, there are a multitude of other properties observable besides the one which is supposed to mark the given class. As in the latter there are, (besides his age) the place of his birth, the nature of his occupation and so on; so in the former there are, (besides its being a statement by a certain kind of

witness) the fact of its being uttered at a certain time and place and under certain circumstances. At the time the statement is made all these qualities or attributes of the statement are present to us and we have a right to take into account as many of them as we please. Now the question to be settled seems to be simply this;—Are the considerations, which we might thus introduce, as immaterial to the result in the case of a witness, as the corresponding considerations are in the case of the insurance of a life? There can surely be no hesitation in the reply to such a question. We soon know all we can know about the prospect of a man's death, and therefore rest content with general statistics of mortality; but no one who heard a witness speak would think of appealing to his figure of veracity. The circumstances under which the statement is made instead of being insignificant are of overwhelming importance. The appearance of the witness, the tone of his voice, the fact of his having objects to gain, together with a countless multitude of other considerations which would gradually come to light, would make any sensible man utterly discard the assigned average. He would, in fact, no more think of judging in this way than he would of appealing to the Northampton tables of mortality to determine the length of life of a soldier who was already in the midst of a battle.

§ 8. It cannot be replied that under these circum-

stances we still refer the witness to a class, and judge of his veracity by an average of a more limited kind; that we infer, for example, that of men who look and act like him under such circumstances, a much larger proportion, say nine-tenths, are found to lie. There is no appeal to a class in this way at all, there is no immediate reference to statistics of any kind whatever. The entire decision seems to depend upon the quickness of the observer's senses and of his apprehension generally. Statistics about the veracity of witnesses seem in fact to be permanently as inappropriate, as all other statistics occasionally may be. We may know accurately the percentage of recoveries after amputation of the leg; but what surgeon would think of forming his judgment solely by such tables when he had a case before him? I do not deny, of course, that the opinion he might form about the patient's prospects of recovery might ultimately rest upon the proportion of deaths and recoveries he might have previously witnessed. But if this were the case, these data are lying, as one may say, obscurely in the background. He does not appeal to them directly and immediately in forming his judgment. There has been a far more important intermediate process of apprehension and estimation of what is essential to the case and what is not. Sharp senses, memory, judgment, and practical sagacity have had to be called into play, and there is not therefore the same direct conscious

and sole appeal to statistics that there was before. The surgeon may have in his mind two or three instances in which the operation performed was equally severe, but in which the patient's constitution was different; the latter element therefore has to be properly allowed for. There may be other instances in which the constitution was similar, but the operation more severe; and so on. Hence, although the ultimate appeal may be to the statistics, it is not so directly; their value has to be estimated through the hazy medium of our judgment and memory, which places them under a very different aspect.

§ 9. The reader will have a good popular illustration of the nature of the difficulty which we have been considering, if he will recall to mind any dispute which he may have heard or taken part in, in which there was an appeal made to the analogy of cases similar to that in dispute. Suppose it were the war in America. A thinks that the North will win because the party which is numerically inferior generally loses. (The appeal here, it should be observed, though not precisely statistical, is still roughly so: the failure occurs 'generally;' in Probability it would be properly assigned in a numerical proportion. But it is an appeal of a fundamentally similar character, and the nature of the argument from it is the same.) B retorts that a numerically inferior party when spread over a vast country generally is not beaten. A urges that

slavery is generally a cause of weakness; not when there is a good feeling between the slaves and their masters, answers B. And so on *ad infinitum*. The reason why no settlement can thus be come to, is, I apprehend, the one given above. There is not here any system of natural classification universally recognized, and appealed to as final, so that there may be general agreement as to the class by the statistics appropriate to which each party is ready to stand or fall. The particular circumstances of the case which may from time to time come into notice, are here of extreme importance from the marked alterations which they produce upon the averages. No one could get up such a dispute if the question were whether a coming child were likely to be a boy or girl.

§ 10. A criticism somewhat resembling the above has been given by Mr Mill (*Logic*, Bk. III. Chap. xviii. § 3) upon the applicability of the theory of Probability to the credibility of witnesses. But he has added other reasons which do not appear to me to be quite valid; he says "common sense would dictate that it is impossible to strike a general average of the veracity, and other qualifications for true testimony, of mankind or any class of them; and if it were possible, such an average would be no guide, the credibility of almost every witness being either below or above the average." The latter objection would apply with equal force to estimating the length of a man's life from

tables of mortality; for the credibility of different witnesses can scarcely have a wider range of variation than the length of different lives. If statistics of credibility could be obtained, they would furnish us in the long run with as accurate inferences as any other statistics, the individual variations of excess and defect being at length neutralised. The original statistics would however be neglected, because there are circumstances in each individual statement which refer it most evidently to some new class depending on different statistics, which latter afford a far better chance of being right in that particular case. In most instances of the kind in question, indeed, such a change is thus produced in the mode of formation of our opinion, that the mental operation ceases to be in any sense founded on a direct appeal to statistics. Another reason moreover for discarding the theory of Probability in these examples is the much greater importance of attaining not merely average truth, but truth in each instance; we had rather not form an opinion at all, than form one which shall only be right in the long run.

The reasons given above seem to me conclusive against the propriety of making testimony and its credibility subjects of Probability. It is not denied, of course, that we may if we please propose questions in this form, and then solve them by the theory. When we do so however, since opinions about real witnesses

are not stated or answered in such a way, I can only regard the expression, 'a witness who lies once in ten times,' as being a sort of synonym for 'a bag yielding a black ball once in ten times,' introduced into the work for the sake of some variety. There are no restrictions whatever upon the right of inventing examples for the sake of illustration, or upon the language in which we may express them. We might, if we chose, assume that two geometrical theorems fail once in ten times, and then determine the chance of a solution being correct, which they both agree in supporting. This would do as well as the example about witnesses, at least as an exercise in arithmetic. But if we were to propose it as a rule for practical guidance, exceptions might begin to be made.

§ 11. There is however a slightly different view of the question which may be taken, and which we must now pause for a moment to examine. Because decisions as to the truth of any *individual* statement cannot reasonably be regarded as belonging to Probability, it does not follow that this science is inapplicable to help us when we have to decide about the truth of a small number of statements. This slight change in the nature and extent of the opinion to be formed does not indeed make any difference in any of the common examples drawn from games of chance, for there the appeal is equally to the statistics, or knowledge of the average, whether we are deciding

about an individual instance or about several; and in each of these cases alike we have only these averages to appeal to. But in other applications of Probability the case is slightly altered. It has been already pointed out that the individual characteristics of any sick man's disease would prevent the surgeon from judging of his recovery by statistics alone and directly; but if an opinion had to be formed about a small number of cases, in a hospital say, statistics and all the inferences they can yield might reasonably be introduced. The ground of this difference is obvious. It arises from the fact that the characteristics of the individual, which made us forsake our original average, do not produce the same disturbance when we have to judge about a group of cases. The original average still remains the most available ground on which to form an opinion, and therefore Probability again becomes applicable.

Now it is conceivable that similar results might follow if we were to alter the range of our observation in the application of Probability to testimony. Because no one ought to judge of the truth of a *single* statement by the rules of that science, does it follow that he ought not thus to judge of the truth of a succession of statements? or, what belongs to the same class of enquiries, that he should not make use of Probability in deciding upon the correctness of the verdict of a jury?

It must be admitted that this application of the theory is not by any means so objectionable as that previously discussed. The individual characteristics of the statement of the witness, or of the circumstances under which it was uttered, which forced us to abandon all appeal to the average of his statements, would not exist in like manner amongst all the witnesses or all the jury. Hence if we could only find out what was the comparative frequency with which true and false statements or judgments were delivered, it would not be so gross an abandonment of the best available sources of information if we were to resolve, in any individual instance, to adhere to this average in the formation of our opinion. Still in the majority of instances, perhaps in all of them, we might find abundantly sufficient data in each separate instance to make an appeal to the average of very inferior value. Political passion, class prejudices, local sympathies, and a multitude of other disturbing agencies of this kind, could generally be detected in such amount as to prevent any one who was not strongly biassed towards statistics from trusting to the mere averages that might be given to him.

CHAPTER XI.

ON THE CAUSES BY WHICH THE PECULIAR SERIES OF PROBABILITY ARE PRODUCED.

§ 1. The characteristic feature of the phenomena to which the theory of Probability is applicable, as these phenomena present themselves when the rules are capable of being immediately applied to them, was fully discussed and illustrated in some of the earlier chapters. We will now enter into a short examination of the causes by which this feature is produced, taking up the enquiry at the point at which it was left in the brief discussion devoted to the subject in the ninth chapter.

§ 2. To divide the sum total of these causes into objects and agencies, is to make a division which, without pretending to absolute philosophical accuracy, will be sufficiently complete for our present purpose. In the tossing up of a penny, for example, the objects would be the penny or pence which were successively thrown ; the agencies, the act of throwing, and every thing which combined with this to make any particular face turn uppermost. This is a very simple and intelligible division, and can easily be extended in

meaning, I think, so as to embrace every class of objects with which Probability is concerned. First then let us suppose a succession of such objects absolutely alike, and let them be exposed to agencies in all respects identical. We should expect to find this identity of antecedents followed by a similar identity of consequents. If similar pence, or the same penny, were thrown in the same way we should expect to find the same face always fall uppermost.

§ 3. But now suppose that instead of actual identity in the antecedents we had a general uniformity with individual variations in the objects, and assume that there is a similar degree of uniformity in the influence of the agencies to which these objects are subjected; it will readily be understood that we may still find in most cases* a uniformity of a similar description in the consequents.

When we come to examine the antecedents in the examples which experience presents to us, this uniformity will be found almost invariably to exist both in the objects themselves, and in the agencies to which

* I say 'in most cases,' because, as every student of the natural sciences is well aware, there are instances in which a very slight difference between the objects, or the agencies to which they are exposed, may bring about a very considerable difference in the result. A difference even of degree only,—for example, temperature,—may cause one of kind,—the qualities of water and ice. But these instances are exceptional; as a general rule we may assume that the resemblance will be perpetuated.

they are exposed. In the case of the objects large classes will be observed throughout all the individual members of which a general resemblance extends. In that of the agencies we shall, it is true, perceive an extreme perplexity. Analysis will show them to be made up of an almost infinite number of different components, but it will detect the same peculiarity, that we have so often had occasion to refer to, pervading almost all these components. The proportions in which they are combined will be found to be nearly, though not quite, the same; the intensity with which they act will be nearly, though not quite, equal. And all these uniformities will unite and blend into a more perfect harmony, according as we take the average of a larger number of instances.

Take, for example, the length of life. According to what we know about the subject, the constitutions of a very large number of persons selected at random will be found to present much the same feature;—general uniformity accompanied by individual irregularity. Now when these persons go out into the world, they are exposed to a variety of agencies, the collective influence of which will assign to each the length of life allotted him. These agencies are very numerous,—climate, food, clothing, &c.—together with many others, the nature of which is at present buried in obscurity. But, owing to their adjustment, the result is that these causes do, in a sort of way, balance each other,

and produce something of uniformity. Each becomes in its turn a cause, is interwoven inextricably with an indefinite number of other causes, and the same kind of uniformity is in this way propagated, amidst endless individual variations, throughout all nature.

§ 4. It may be said that this statement is no answer to the question with which we started, for instead of explaining how a certain state of things is caused, it points out that the same state exists elsewhere. There is a uniformity in the objects when they are submitted to calculation; we then grope about amongst the causes of them, and after all only discover a precisely similar uniformity existing amongst those causes. This is perfectly true, and indeed nothing else could have been fairly expected. Ultimately no cause *can* be assigned, because there is none to assign; we can only state it as an experimental fact that such is actually the arrangement of things. Mediately, however, we can do something, by pointing out the means through which the derivative results are obtained. Taking this as the object of our enquiry, we have seen, by the above analysis, that the uniformity in question may be principally assigned to two causes or rather conditions.

§ 5. (I.) There are classes of objects, each class containing a multitude of individuals more or less resembling one another. Suppose that the phenomenon under consideration is the length of life. The objects

in this case, are the human beings whose lives we are considering. The resemblance existing amongst them is to be found in the strength and soundness of the organs which they possess, together with all the circumstances which collectively make up what we call the goodness of their constitutions. The uniformity that we may trace in the results is owing, much more than is often suspected, to this arrangement of things in natural kinds, each kind containing a large number of individuals. Were each kind of animals limited to a single pair, or even to but a few pairs, there would not be much scope left for the collection of statistical tables amongst them. Or, to make a less violent supposition;—If the numbers in each natural class of objects were much smaller than they are at present, and the distinctions between the classes somewhat more marked, the consequent inapplicability of any kind of statistical tables to them, though not quite fatal, would still be very serious. A large number of objects in the class, together with that general similarity which entitles the objects to be fairly comprised in one class, seem to be important conditions for the applicability of the theory of Probability to any phenomena.

§ 6. (II.) By the adjustment of the relative intensity of the different forces and agencies in nature, and the respective frequency of their occurrence, the effects which these produce are also tolerably uniform.

It is quite conceivable that this second condition should correct the former by converting this general uniformity into an absolute one, or that on the other hand it should aggravate it into utter want of uniformity. As a matter of fact this second condition does neither, but simply varies the details, leaving the uniformity of precisely the same general description as it was before. Or, if the objects are supposed to be absolutely alike, as in the case of successive throws of a penny when the same penny is always thrown, it may serve to create this kind of uniformity. Thus, to recur to a former instance; One man overworks himself, another follows an unhealthy trade, a third exposes himself to infection, &c.; the result of all this is that the length of men's lives, like the strength of their constitutions, preserves, when tabulated, a tolerable regularity.

§ 7. The reader must observe that this condition is arbitrary, in the same sense in which the former condition was arbitrary, and in a greater degree. We not merely can conceive the absence of such uniformity in the agencies; we can easily find instances in which uniformity is actually wanting. Thus the length of life is tolerably regular, and so are the numbers who die in successive years or centuries of most of the common complaints. But is it so of all diseases? What of the Sweating sickness, the Black death, the Asiatic cholera? I am not denying that these events have their causes,

and that they would be produced again by the recurrence of the conditions which caused them before. But they do not recur; at least not the former diseases. They seem to have depended upon such rare conditions that their occurrence was almost solitary; and when they did occur their course was so excentric and irregular as to entirely deprive their results (that is, the number of deaths which they caused) of the statistical uniformity of which we are speaking.

We can only lay it down therefore as a general, not a universal rule, that the agencies in question show the kind of uniformity which is requisite to make the objects affected by them fitting subjects of Probability. It may be replied that the occasional irregularity just alluded to only arises from the fact of our having confined ourselves to too limited a time, and that we shall see it disappear here, as elsewhere, if we keep our tables open long enough. This reply is conclusive only upon the supposition that the ways and thoughts of men are, in the long run, invariable, or subject only to periodic changes. On the assumption of a steady progress in society either for the better or the worse, the argument falls to the ground at once. From what we know of the course of the world these fearful pests of the past may be considered as solitary events in our history, or at least events which will not be repeated. No uniformity would therefore be found in the deaths which they occasion, though the registrar's

books should be kept open for a million years, and these agencies are therefore for the most part excluded from the science of Probability.

§ 8. Having thus examined the process by which the results in question are brought about, it may now be interesting to spend a short time in the enquiry, What are the principal classes of things amongst which such conditions are to be discovered?

(I.) These conditions prevail principally, I apprehend, in the properties of natural kinds; both in the ultimate and in the accidental and derivative properties. In all the characteristics of natural species;—in all they do, and in all which happens to them, so far as it depends upon their properties—we seldom fail to discover this regularity. Thus in men, their height, strength, weight; the age to which they live, the deaths of which they die; all present a well-known uniformity. Life Insurance tables offer a good instance of the multiplicity and importance of the above-mentioned applications of Probability.

(II.) The same peculiarity prevails in the force and frequency of most natural agencies. Winds and storms are seen to lose their proverbial irregularity when examined on a large scale. Man's work therefore when operated on by such agencies as these, even though it had been made in different cases absolutely alike to begin with, afterwards shows only a general regularity. I may sow exactly the same amount of

seed in my field every year. One season the yield may be moderate, the next be extraordinarily abundant through hot dry weather, and the third be much injured by hail. But in the long run these irregularities will disappear in the result of my crops, because they disappear in the power and frequency of the productive agencies. The business of underwriters, Fire Insurance, &c., will fall principally under this head, though in some respects they are more connected with the former. But the distinction which is thus made, is, after all, principally one of arrangement; these natural agencies are closely assimilated to the properties of natural kinds, and indeed might perhaps be considered such, if we were to extend our observations.

§ 9. The above are instances of natural objects and natural agencies. I am inclined to believe that it is in such only, as distinguished from things artificial, that the property in question is to be found. This is an assertion that will need some discussion and explanation. Two instances, in apparent opposition, will at once occur to the mind of some readers, one of which from its great intrinsic importance, and the other from the frequency of the problems which it furnishes, will demand separate examination.

§ 10. (1) In the course of observation, by astronomical and other instruments, the utmost possible degree of accuracy is often desired, a degree which

cannot be attained by any one single observation. What we do therefore in these cases is to make a very large number of different observations, which are naturally found to differ somewhat from one another in their results; by means of these the true value is to be found as accurately as possible. This process is one which astronomers have such constant occasion to perform that a special rule (that known as the rule of least squares) has been invented for the purpose. I have already alluded to this rule in a former chapter, and need only say at this point that its object is to determine the unknown true result from a considerable number of the known but slighty incorrect results.

The subjects then of calculation here are a certain number of elements—slightly incorrect elements—given by successive observations. Are not then these observations artificial, or the direct product of voluntary agency? I think not; at least it rather depends on what we understand by voluntary. What is really intended and aimed at by the observer is, of course, perfect accuracy, that is, the true observation, or the voluntary steps and preliminaries on which this observation depends. Whether voluntary or not this result only can be called intentional. But this result is not obtained. What we actually get in its place is a series of deviations from it, containing results more or less wide of the truth. Now by what are these deviations caused? It appears to me that

agencies are at work here, similar for the most part to those whose operation we have just been considering in some of the previous sections. Heat, dust, friction, draughts of air, are some of the causes which divert us from the truth. Besides these there are other causes which certainly depend upon human agency, but which are not strictly speaking voluntary; on the contrary they owe the character they possess, of general uniformity only as opposed to absolute uniformity, to the fact of their being involuntary. They are such as the irregular action of the muscles, inability to make our organs execute the precise purposes we have in mind, &c. All these conditions, though utterly incalculable singly, vary in the long run tolerably uniformly.

§ 11. A few words may here be added to the remarks made in the second chapter upon this rule of Least Squares. It would be presumptuous in me to attempt any criticism of the mathematics themselves by which the rule is supported or proved, I only wish to make it plain why I cannot regard the mathematics as necessarily rigidly appropriate. The possibility of determining the correct observation, by the help of any formula, when the actual data before us are a series of slightly incorrect observations, seems to imply that these incorrect elements will group themselves in some determinate and orderly way about the correct one. And the possibility of determining the requisite

formula *à priori* implies that the law according to which the elements thus group themselves must be the same or similar under all possible different circumstances. It was urged in a former chapter, that if this state of things did really exist, it would seem that a great extent of special experience would be necessary to verify it. Now that we have made a brief examination of the different agencies by which any uniformity of the kind in question is generally produced in the cases in which it does actually exist, there seems still more reason for such an appeal to experience. The series to which Probability is applied are generally the result of a combination of many and complicated agencies, and these agencies often show a tendency, under certain circumstances, so to change their effects that the rules of Probability may at length become inappropriate. Unless then the series of observations to which the rule of Least Squares is applied are entirely unlike most of the other natural series in which Probability is made use of (a conclusion which I have attempted to disprove above), we surely ought to insist upon something more than the purely abstract *à priori* principles which are commonly offered in support of the rule.

§ 12. (2) The other example to which I allude is the stock one of cards and dice. Here, as in the last case, the result is remotely voluntary, in the sense that it would not be produced at all but for human

agency. But mediately the result is produced by so many involuntary agencies that it owes its characteristic properties to these. The turning up, for example, of a particular face of a die is the result of voluntary agency, but it is not an immediate result. That particular face was not chosen, though the fact of its being chosen may be the remote consequence of an act of choice. There has been an intermediate chaos of conflicting agencies, which no one can calculate before or distinguish afterwards. These agencies seem to show a uniformity in the long run, and thence to produce a similar uniformity in the result.

§ 13. The distinction here insisted on may seem to some persons a needless one, but I think that serious errors have arisen from neglecting it. The immediate products of man's mind, so far as we can obtain them, do not seem to possess this essential characteristic of Probability. Their characteristic seems rather to be, either perfect mathematical accuracy or utter want of it; either law unfailing, or no law whatever. Practically the mind has to work by the aid of imperfect instruments, and under the influence of various and conflicting agencies, and by these means its work ultimately loses its original properties. If a man, for example, instead of producing numerical results by imperfect observations, thinks of numbers at once, what sort of series does he obtain? One about as far removed from the series to which we are accustomed

in observations as can well be imagined. Or take another product of human efforts, in which the intention can be executed with tolerable success. When any one builds a row of houses, there are disturbing influences at work (shrinking of bricks and mortar, settling of foundations, &c.). But the effect which these disturbances are able to produce is inappreciably small, and we may consider therefore that the result obtained is the direct product of the mind, that is, the accurate realization of its intention. And what is the consequence? Every house in the row is exactly the same height, width, &c. as the others; or if there are variations, they are few, definite, and regular. The result offers no resemblance whatever to the height, weight, &c. of a number of men selected at random. The builder probably had some regular design in contemplation, and he has succeeded in executing it.

§ 14. It may be replied that if we extend our observation, say to the houses of a large city, we shall then detect the property under discussion. The proportion of the different heights would resemble that of the heights of a large number of men. Possibly it might, though even then I think the resemblance would be far from being a close one. But this is to wander on to other ground. I am not speaking of the work of different minds, but of that of one mind. In a multiplicity of designs there may be that variable

uniformity which is not to be found in a single design. So far as this is the case, however, it would be a return to the principles laid down in the opening of this chapter. The height which the *different* builders contemplated might be found to group themselves into something of the same kind of uniformity as that which prevails in most other things which they should undertake to do independently. We might then trace the action of the same two conditions;—a harmony in the multitude of their different designs, a harmony also in the infinite variety of the influences which have modified those designs. But this is a very different thing from saying that the work of one man will show such a result as this. The difference is much like that between the tread of a thousand men who are stepping without thinking of each other, and their tread when they are drilled into a regiment. In the former case there is the working of a thousand minds, in the latter of one only. The former therefore would introduce us to the province of Probability, the other would not.

§ 15. The foregoing very brief enquiry into the causes by which the peculiar form of statistical results, with which we have been throughout concerned, are actually produced, must suffice here. Any fuller discussion would seem to belong more properly to a far wider science, in fact to the general philosophy of Inductive evidence. The conditions upon which the

production of our general statistical propositions depends, as distinguished from the inferences to be made from them when they are obtained, lie outside the confines of the science of Probability.

CHAPTER XII.

FALLACIES.

§ 1. In works on Logic a chapter is generally devoted to the discussion of Fallacies, that is, to the description and classification of the different ways in which the rules of Logic may be transgressed. The analogy of Probability to Logic is sufficiently close to make it advisable to adopt the same plan here. In describing my own opinions I have been, of course, often obliged to describe and criticize those of others when they seemed erroneous. But some of the most widely spread errors find no supporters worth mentioning, and exist only in vague popular misapprehension. It will be found the best arrangement, I think, at the risk of occasional repetition, to collect and classify a few of the errors that occur most frequently, and as far as possible to trace them to their sources. In doing so I shall for the most part confine myself to the special province of this work, the application, namely, of Probability to moral and social science, and shall avoid the discussion of isolated problems in games of chance and skill except when some error of principle seems to be involved in them.

§ 2. (I.) One of the most fertile sources of error and confusion upon the subject has been already several times alluded to, and in part discussed in a previous chapter. This consists in choosing the class to which to refer an event, and therefore judging of the rarity of the event and the consequent improbability of foretelling it, *after it has happened*, and then transferring the impressions we experience to a supposed contemplation of the event beforehand. No error need arise in this way if we were careful as to the class which we thus selected; but such carefulness is often neglected.

An illustration may serve to make this plain. A man once pointed to a small target chalked upon a door, the target having a bullet hole through the centre of it, and surprised some spectators by declaring that he had fired that shot from an old fowling-piece at a distance of a hundred yards. His statement was true enough, but he suppressed a rather important fact. The shot had really been aimed in a general way at the barn door, and had hit it; the target was afterwards chalked round the spot where the bullet struck. A deception analogous to this is, I think, often practised unconsciously in other matters. We judge of events on a similar principle, feeling and expressing surprise in an equally unreasonable way, and deciding as to their occurrence on grounds which are really merely a subsequent adjunct of our own.

Butler's remarks about the story of Cæsar, discussed already in the fifth chapter, are of this character. He selects a series of events from history, and then imagines a person guessing these correctly who at the time has not the history before him. As I have already pointed out, it is one thing to be unlikely to guess an event rightly without specific evidence; it is another and very different thing to judge of the truth of a story which was founded upon evidence. But it is a great mistake to transfer to one of these ways of viewing the matter the mental impressions which properly belong to the other. It is like drawing the target afterwards, and then being surprised that the shot lies in the centre of it.

§ 3. One aspect of this fallacy has been already discussed, but it will serve to clear up difficulties which are often felt upon the subject if we re-examine the question under a somewhat more general form.

In the class of examples under discussion we are generally presented with an individual which is not indeed definitely referred to a class, but in regard to which we have generally no difficulty in choosing the appropriate class. Now suppose we were contemplating such an event as the throwing of sixes with a pair of dice four times running. Such a throw would be termed a very unlikely event, and the odds against its happening would be said to be $36\times36\times36\times36-1$

to 1 or 1679615 to 1. The meaning of these phrases, as has been abundantly pointed out, is simply that the event in question occurs very rarely; stated with numerical accuracy, it occurs once in 1679616 times.

§ 4. But now let us make the assumption that the throw has actually occurred; let us put ourselves into the position of contemplating sixes four times running, when it is supposed to be known or reported that this throw has happened. The same phrase, namely that the event is a very unlikely one, will often be used in relation to it, but we shall find that this phrase introduces extremely different meanings. Properly speaking Probability is scarcely applicable then; the throw in question being supposed to have happened, things are in a stage in relation to that throw in which all inferences from the science of Probability are superseded. The event is known, and therefore we need not now judge by means of statistics as to what might have been expected to occur. When however, as is often the case, Probability is appealed to in reference to such a throw, we shall find that two or three quite distinct meanings are introduced.

§ 5. (1) There is, firstly, the most correct meaning. The event, it is true, has happened, and we know what it is, and therefore, as just stated, we have not really any occasion to resort to the rules of Pro-

bability; but we can nevertheless conceive ourselves as being in the position of a person who does not know, and who has only Probability to appeal to. By calling the chances 1679615 to 1 against the throw we then mean to state the fact, that inasmuch as such a throw occurs only once in 1679616 times, our guess, were we to guess, would be correct only once in the same number of times, that is if it were a fair guess simply on these statistical grounds.

§ 6. (2) But there is a second and very different conception sometimes introduced, especially when the event in question is supposed to be known, not as above by the evidence of our experience, but by the report of a witness. We may then mean by the 'chances against the event' (as was pointed out in Chapter v.) not the proportional number of times we should be right in guessing the event, but the proportional number of times the witness will be right in reporting it. The grounds of our inference are here shifted altogether. In the former case the statistics were the throws and their respective frequency, now they are the witnesses' statements and their respective truthfulness.

§ 7. (3) But there is yet another meaning sometimes intended to be conveyed when persons talk of the chances against such an event as the throw in question. They may mean—not, Here is an event,

how often should I have guessed it?—nor, Here is a report, how often will it be correct?—but something entirely different from either, namely, Here is an event, how often will it be found to be produced by some one particular cause?

This meaning will often be found to introduce itself in the case of coincidences. When, for example, a man hears of dice giving as above the same throw several times running, and speaks of this as very extraordinary, we shall often find that he is not merely thinking of the improbability of his guess being right, or the report being true, but, along with this, of the throw having been produced by fair dice*. There is, of course, no reason whatever why such a question as this should not be referred to Probability, provided always that we could find the appropriate statistics by which to judge. These statistics would be composed, not of throws of the particular dice, nor of reports of the particular witness, but of the occasions on which such a throw as the one in question respectively had and had not been produced fairly. The objection to this view of the question would be that no such statistics are obtainable, and if they were, we should prefer to form our opinion (on principles described in Chapter IX.) from the special circumstances of the case rather than from an appeal to the average.

* There are some remarks on this in Mill's *Logic*, Bk. III. ch. XXV.

§ 8. The reader will easily be able to supply any number of examples in illustration of the distinctions just given; we will briefly examine but one. I hide a banknote in a certain book in a large library, and leave the room. A person tells me that after I went out a stranger came in, walked straight up to that particular book, and took it away with him. Many people on hearing this account would reply, How extremely improbable! On analysing the phrase, I think we shall find that certainly two, and possibly all three, of the above meanings are involved in this exclamation. (1) What may be meant is this,—Assuming that the report is true, and the stranger innocent, a rare event has occurred. Many books might have been thus taken without that particular one being selected. I should not therefore have expected the event, and when it has happened I am surprised. Now a man has a perfect right to be surprised if he pleases, but he has no logical right (*on this view*) to make his surprise a ground for disbelieving the event. To do this is to fall into the fallacy described at the commencement of this chapter. The fact of my not having been likely to have guessed a thing beforehand is no reason for doubting it when I am told of it. (2) Or I may stop short of the events reported, and apply the rules of Probability to the report itself. If so, what I mean is, as has been several times described, such a story as this now before me is of a kind very

generally false, and I cannot therefore attach much credit to it now. (3) Or I may accept the truth of the report, but doubt the fact of the stranger having taken the book at random. If so, what I mean is that of men who take books in the way described, only a small proportion will be found to have taken them really at random; the majority will do so because they had by some means found out what there was inside the book.

§ 9. Each of the above three meanings is a possible and a legitimate meaning. The only requisite is that we should be very careful to ascertain which of them is present to the mind, so as to select the appropriate statistics. The first makes in itself the most legitimate use of Probability; the only drawback being that at the precise time in question the functions of Probability are superseded by the event being otherwise known*. The second or third, therefore, is the more likely meaning to be present to the mind, for in these cases Probability, if it could be practically made use of, would, at the time in question, be a means of drawing really important inferences. The drawbacks here, are the impossibility of finding such statistics, and the extreme disturbing influence upon these sta-

* The fallacy described at the commencement of this Chapter consists in failing to observe this, and appealing to the statistics appropriate to the first meaning to draw a conclusion which ought to rest on those appropriate to the second or third.

tistics of the circumstances of the special case. Although, therefore, we frequently draw conclusions in such a case on the principles of the science, we cannot do this with any such approach to accuracy as would justify us in obtaining numerical results.

§ 10. (II.) The fallacy described at the commencement of this chapter arose from determining to judge of an observed or reported event by the rules of Probability, but employing an entirely wrong set of statistics in the process of judging. Another fallacy, closely connected with this, arises from the practice of taking some only of the characteristics of such an event, and arbitrarily confining to these the appeal to Probability. An example may serve to make this plain. I toss up twelve pence, and find that eleven of them give heads. Many persons on witnessing this would experience a feeling which they would express by the remark, How near that was to getting all heads! And if any thing very important were staked on the throw they would be much excited at the occurrence. But in what sense were we near to twelve? The number eleven of course is nearer to twelve than ten or nine are, but there is surely something more than this in the person's mind at the moment. There is a not uncommon error, I apprehend, which consists in unconsciously regarding the eleven heads as a thing which is already somehow secured so that one might, as it were, keep them and then take our chance for

the odd one. The eleven are mentally set aside, looked upon as certain (for they have already happened) and we then introduce the notion of chance merely for the twelfth. But this twelfth, having also happened, has no better claim to such a distinction than any of the others. If we will introduce the notion of chance in the case of the one that gave tail we must do the same in the case of all the others. In other words, if the tosser be dissatisfied at the appearance of the one tail, and wish to cancel it and try his luck again, he must toss up the whole lot of pence again fairly together. In this case, of course, so far from his having a better prospect for the next throw he may think himself in very good luck if he makes again as good a throw as the one he rejected.

§ 11. In the above example the error is so transparent that a very slight amount of reflection will enable any one to see through it. But in forming a judgment upon matters of greater complexity than dice and pence, especially in the case of what are called 'narrow escapes,' a mistake of an analogous kind is, I apprehend, far from uncommon. A person, for example, who has just experienced a narrow escape will often be filled with surprise and anxiety amounting almost to terror. The event being past, these feelings are, at the time, in strictness inappropriate. If, as is quite possible, they are merely instinctive, or the result of association, they do not fall within the

province of any kind of Logic. If however, as seems to me far more likely, they arise from a supposed transference of ourselves into that point of past time at which the event was just about to happen, and the production by imagination of the feelings we should then expect to experience, this process partakes of the nature of an inference, and can be right or wrong. In other words, the alarm may be proportionate or disproportionate to the amount of danger that might fairly have been reckoned upon in such a hypothetical anticipation. If we attend to the remarks people make on such occasions, we shall find, I think, that they do distinctly consider that their feelings admit of justification; if so, I do not perceive by what other process they can be justified than by that which has been just described. If the supposed transfer were completely carried out, there would be no fallacy; but it is often very incompletely done, some of the component parts of the event being supposed to be determined or 'arranged' (to use a sporting phrase) in the form in which we now know that they actually have happened, and only the remaining ones being fairly contemplated as future chances by Probability.

A man, for example, is out with a friend, whose rifle goes off by accident, and the bullet passes through his hat. He trembles with anxiety at thinking what might have happened, and perhaps remarks, 'How very near I was to being killed!' Now we

may safely assume that he means something more than that a shot passed very close to him; such an event might have been produced purposely and cautiously by his friend, and in that case his feelings would have been totally different. He has now some vague idea that, as he would probably say, 'his chance of being killed then was very great.' His surprise and terror may be in great part physical and instinctive, arising simply from the knowledge that the shot had passed very near him. But his mental state may be analysed, and I think we shall find, at bottom, a fallacy of the kind described above. To speak or think of chance in connection with the incident, or to refer to what might have been, is to refer the particular incident to a class of incidents of a similar character, and then to consider the comparative frequency with which the contemplated result ensues. Now the series which we may suppose to be most naturally selected in this case is one composed of shooting excursions with his friend; up to this point the proceedings are assumed to be designed, beyond it only, in the subsequent event, was there accident. Once in a million times perhaps on such occasions the gun will go off accidentally; one in a thousand only of those discharges will be directed near his friend's head. If we will make the accident a matter of Probability, we ought by rights in this way (to adopt the language of the first example), to 'toss up again' fairly. But we do

not do this; we seem to assume for certain that the shot goes within an inch of our heads, detach that from the notion of chance at all, and then begin to introduce this notion again for possible deflections from that saving inch. In such a case one's prospects naturally become very disagreeable.

§ 12. If the reader will try to analyse his feelings just after he has had a narrow escape himself, or witnessed one in others, I think he will find that this fallacy is generally to some extent involved in them. The mere proximity to danger cannot be the cause of the anxiety, for in other cases where the danger was equally near, but from which the notion of chance is excluded, no such anxiety is felt. I do not think that any one can make any justification of his feelings, or would naturally attempt to make one, without introducing the notion of chance. We shall find it scarcely possible to explain what we feel without introducing an "if," or putting ourselves mentally into the same position again, and then thinking about the different issues of the event which might be expected as a general rule under those circumstances. If in such a position the probability of danger would be really great, terror would not have been inappropriate then, and therefore anxiety is a very legitimate product of imagination afterwards. But if, on the other hand, as is very often the case when all the chances are contemplated, even that amount of proximity to

danger which was actually experienced was extremely improbable, then no justification can be offered for the subsequent anxiety.

§ 13. (III.) A common mistake is to assume that a very unlikely thing will not happen at all. It is a mistake which, when thus stated in words, is too obvious to be committed, for the meaning of an unlikely thing is one that happens at rare intervals; if it were not certain that the event would happen at rare intervals it would not be called unlikely. This is an error that could only occur in vague popular misapprehension, and is so abundantly refuted in other works on Probability, that I shall touch upon it very briefly here. It follows of course, from our definition of Probability, that to speak of a very rare combination of events as one that is 'sure never to happen,' is to use language incorrectly. Such a phrase may pass as popular exaggeration, but otherwise it is either tautological or contradictory. The truth about such rare events cannot be better described than in the following quotation from De Morgan*:—

"It is said that no person ever *does* arrive at such extremely improbable cases as the one just cited [drawing the same ball five times running out of a bag containing twenty balls]. That a given individual should never throw an ace twelve times running on a single die, is by far the most likely; indeed, so remote are

* *Essay on Probabilities*, p. 126.

the chances of such an event in any twelve trials (more than 2,000,000,000 to 1 against it) that it is unlikely the experience of any given country, in any given century, should furnish it. But let us stop for a moment, and ask ourselves to what this argument applies. A person who rarely touches dice will hardly believe that doublets sometimes occur three times running; one who handles them frequently knows that such is sometimes the fact. Every very practised user of those implements has seen still rarer sequences. Now suppose that a society of persons had thrown the dice so often as to secure a run of six aces observed and recorded, the preceding argument would still be used against twelve. And if another society had practised long enough to see twelve aces following each other, they might still employ the same method of doubting as to a run of twenty-four, and so on, *ad infinitum*. The power of imagining cases which contain long combinations so much exceeds that of exhibiting and arranging them, that it is easy to assign a telegraph which should make a separate signal for every grain of sand in a globe as large as the visible universe, upon the hypothesis of the most space-penetrating astronomer. The fallacy of the preceding objection lies in supposing events in number beyond our experience, composed entirely of sequences such as fall within our experience. It makes the past necessarily contain the whole, as to the quality of its components; and judges

by samples. Now the least cautious buyer of grain requires to examine a handful before he judges of a bushel, and a bushel before he judges of a load. But relatively to such enormous numbers of combinations as are frequently proposed, our experience does not deserve the title of a handful as compared with a bushel, or even of a single grain*."

§ 14. The origin of this inveterate mistake is not difficult to be accounted for. It arises, no doubt, from the exigencies of our practical life. No man can bear in mind every contingency to which he may be exposed. If therefore we are ever to do anything at all in the world, a large number of the rarer contingencies must be entirely left out of account. And the necessity of this oblivion is strengthened by the shortness of our life. Mathematically speaking it would be said to be certain that any one who lives long enough will be bitten by a mad dog, for the event is not an impossible, but only an improbable one, and must therefore come to pass in time. But this and an indefinite number of other disagreeable contingencies have on most occasions to be entirely ignored in practice, and thence they come almost necessarily to drop equally out of our thought and expectation. And when the event is one in itself of no importance, like a rare throw of the dice, it requires almost an effort of imagi-

* *Essay on Probabilities*, p. 126.

nation to some persons to realise the throw as being even possible.

§ 15. There is one particular form of this error which, from the importance attached to it by some writers, deserves perhaps more special examination. As stated above, there can be no doubt that, however unlikely an event may be, if we (loosely speaking) vary the circumstances sufficiently, or if, in other words, we keep on trying long enough, we shall meet with such an event at last. If we toss up a pair of dice a few times we shall get doublets; if we try longer with three we shall get triplets, and so on. However unusual the event may be, even were it sixes a thousand times running, it will come some time or other if we have only patience and vitality enough. Now apply this result to the letters of the alphabet. Suppose that one at a time is drawn from a bag which contains them all, and is then replaced. If the letters were written down one after another as they occurred, it would commonly be expected that they would be found to make mere nonsense, and would never arrange themselves into the words of any language known to men. No more they would in general, but it is a commonly accepted result of the theory, and one which we may assume the reader to be ready to admit without further discussion, that, if the process were continued long enough, words making sense would appear; nay more, that any book we chose to

mention,—Milton or Shakespeare, for example,—would be produced in this way at last. It might take more years than we have space in this volume to represent in figures to obtain such works, but come they would at last. Now many people have not unnaturally thought it derogatory to genius to suggest that its productions could have also been obtained by chance, whilst others have gone on to argue, If this be the case, might not the world itself in this manner have been produced by chance?

§ 16. *Dugald Stewart makes a reference to some remarks of Condorcet upon this subject, and has thought his reasonings of sufficient importance to need a detailed criticism. I will only remark here on this particular point, that any such inference as this about the creation of the world, involves in addition the fallacy described in § 9. It confounds together the probability of our foretelling an event with the probability of the event having been produced in some given way. But the other inference,—that, I mean, about the production of a Shakespeare,—seems equally startling and capable of leading to identical conclusions, whilst its meaning is unmistakable, and its truth not likely to be disputed by students of Probability.

It may console some persons to be reminded that

* Dugald Stewart's *Works*, edited by Sir W. Hamilton, Vol. VII. p. 115.

the power of producing a Shakespeare, *in time*, is not confined to consummate genius and to mere chance. Any one, down almost to an idiot, might do it, if he took sufficient time about the task. For suppose that the required number of letters were arranged, not by chance but designedly, and according to the rules of the theory of permutations: their number being really finite, every order in which they could occur would come in its due turn, and therefore every thing which can be expressed in language would be arrived at some time or other, the works of Shakespeare of course amongst other things. It would probably take about as long to do it one way as the other, but with unlimited time either plan would be feasible.

§ 17. There is really nothing that need startle or shock any one in such a theory. It arises from the following cause. The number of letters, and therefore of words at our disposal is limited; when therefore anything is to be expressed in language it necessarily becomes subject to this limitation. The possible variations of thought are literally infinite, so are those of spoken language (by intonation of the voice, &c.); but when we come to words there is a limitation which is distinctly conceivable by the mind, though the restriction is one that in practice will never be appreciable. The answer therefore is plain, and it is one that will apply to many other cases as well, that

to put a finite limit upon the number of ways in which a thing can be done is to determine that any one who is able and willing to try long enough shall succeed in doing it. If a great genius condescends to do it under these circumstances, he must submit to the possibility of having his claims disputed by the chance-man or idiot. If Shakespeare were limited to ten words, the time within which the latter agents might claim equality with him need not be very great. As it is, having the range of the English language at his disposal, his reputation is not in any present danger of being assailed on such grounds.

As an additional security it may be remarked that each of these latter agents, even when in course of time they had stumbled upon a brilliant conception, would probably not be aware of the fact, so that practically they would require a Shakespeare at their elbow to tell them at which of their performances they had better stop.

§ 18. (IV.) In discussing the nature of the connexion between Probability and Induction, we examined the claims of a rule commonly given for inferring the probability that an event which had been repeatedly observed would recur again. I endeavoured to show that all attempts to obtain and prove such a rule were necessarily futile; if these reasons were conclusive the employment of such a rule must of course be regarded as fallacious. There

is no necessity to repeat here the arguments which were employed on a former occasion; I will only recall the reader's attention to the following considerations, which were then but very briefly touched upon.

Instead of there being one single rule of succession we might divide the possible forms of the rule into three classes.

§ 19. (1) In some cases when a thing has been observed to happen several times it becomes in consequence *more* likely that the thing should happen again. This agrees with the ordinary form of the rule, and is probably the case of most frequent occurrence. The necessary vagueness of expression when we talk of the 'happening of a thing' makes it quite impossible to tolerate the rule in this general form, but if we specialize it a little we shall find it assume a more familiar shape. If, for example, we have observed two or more properties to be frequently associated together in a succession of individuals, we shall conclude with some force that they will be found to be so connected in future. The strength of our conviction however will depend not merely on the number of observed coincidences, but on far more complicated considerations; for a discussion of which the reader must be referred to regular treatises on Inductive evidence. Or again, if we have observed one of two events succeed another several times, the occurrence of the former will excite in most cases

some degree of expectation of the latter. As before, however, the degree of our expectation is not to be assigned by any simple formula; it will depend in part upon the supposed intimacy with which the events are connected. This would lead to a discussion upon laws of causation, and the circumstances under which their existence may be inferred.

§ 20. (2) Or, secondly, the past recurrence may in itself give no valid grounds for inference about the future; this is the case which most properly belongs to Probability. That it does so belong will be easily seen if we bear in mind the fundamental conception of the science. We are there introduced to a series,—for purposes of inference an indefinitely extended series,—of terms, about the details of which information is not given; our knowledge being confined to the statistical fact, that, say, one in ten of them has some attribute which we will call X. Suppose now that five of these terms in succession have been X, what hint does this give about the sixth being also an X? Clearly none at all; this past fact tells us nothing; the formula for our inference is still precisely what it was before, that one in ten being X it is one to nine that the next term is X. And however many terms in succession had been of one kind, precisely the same formula would still be given.

§ 21. The way in which events will justify the answer given by this formula is often misunderstood.

For the benefit therefore of those unacquainted with some of the conceptions familiar to mathematicians, I will add a few words of explanation. Suppose then that we have had X twelve times in succession. This is clearly an anomalous state of things. To suppose anything like it to continue for ever would be obviously in opposition to the statistics, which assert that in the long run only one in ten is X. But how is this anomaly got over? In other words, how do we obviate the conclusion that X's will occur more frequently than once in ten times, after such a long succession of them as we have now had? Many people seem to believe that there must be a diminution of X's afterwards to counterbalance their past preponderance. This however would be quite a mistake; the proportion in which they occur in future must remain the same throughout; it cannot be altered if we adhere to our statistical formula. The fact is that the rectification of the exceptional disturbance in the proportion will be brought about simply by the continual influx of fresh terms in the series. These will in the long run neutralize the disturbance, not by any special adaptation, as it were, for the purpose, but by the mere weight of their overwhelming numbers. At every stage therefore, in the succession, whatever might have been the number and nature of the preceding terms, it will still be true to say that one in ten of the terms will be an X.

If we had to do only with a finite number of terms, however large that number might be, such a disturbance as we have spoken of would, it is true, need a special alteration in the subsequent proportions to neutralize its effects. But when we have to do with an infinite number of terms, this is not the case; the 'limit' of the series, which is what we then have to deal with (Ch. III. § 33), is entirely unaffected by these temporary disturbances. In the continued evolution of the series we shall find, as a matter of fact, more and more of such disturbances, and these of a more and more exceptional character. But whatever the point we may occupy at any time, if we look forward or backward into the indefinite extension of the series, we shall still see that the ultimate limit to the proportion in which its terms are arranged remains the same; and it is with this limit, as above mentioned, that we are concerned in the strict rules of Probability. Suppose, for illustration, that there is one part in a thousand of salt in sea-water. Put some fresh water in a cistern; then however much there may be of the fresh, if only salt water enough is turned on, the only proportion that will be ultimately approximated to will be the same as that in the sea itself, viz. one part in a thousand of salt. To effect this it will not be necessary to suppose the subsequent sea-water to be more salt; the proportion will be restored, or, to speak strictly, will

tend to be restored simply by adding for ever water of the original degree of saltness.

The commonest example, perhaps, of this kind is that of tossing up a penny. Suppose we have had four heads in succession; people have tolerably realized by now that 'head the fifth time' is still an even chance, as 'head' was each time before, and will be ever after. The preceding paragraph explains how it is that these occasional disturbances in the average become neutralized in the long run.

§ 22. (3) There are other cases which, though rare, are by no means unknown, in which such an inference as that obtained from the Rule of Succession would be the direct reverse of the truth. The oftener a thing happens, it may be, the more unlikely it is to happen again. This is the case whenever we are drawing things from a limited source (as balls from a bag), or whenever the act of repetition itself tends to prevent the succession (as in giving false alarms).

I am quite ready to admit that we believe the results described in the last two classes on the strength of some such general Inductive rule, or rather principle, as that involved in the first. But it would be a great error to confound this with an admission of the universal validity of the rule in each special instance. We are speaking about the application of the rule to individual cases, or classes of cases; this is quite a distinct thing, as was pointed out in a previous

chapter, from giving the grounds on which we rest the rule itself. If a man were to lay it down as a universal rule, that the testimony of all persons was to be believed, and we adduced an instance of a man having lied, it would not be considered that he saved his rule by shewing that we believed that it was a lie on the word of other persons. But it is perfectly consistent to give as a general (not a universal) rule, that the testimony of men is credible, then to separate off a second class of men whose word is not to be trusted, and finally, if any one wants to know our ground for the second rule, to rest it upon the first. If we were speaking of *necessary* laws, such a conflict as this would be as hopeless as the old 'Cretan' dilemma; but in instances of Inductive and Analogical extension it is perfectly harmless.

§ 23. A familiar example, about which many people must have disputed at one time or another of their lives, will serve to bring out the three different possible conclusions mentioned above. We have observed it rain on ten successive days. *A* and *B* conclude respectively for and against rain on the eleventh day; *C* maintains that the past rain affords no data whatever for an opinion. Which is right? We really cannot determine *à priori*. An appeal must be made to direct observation, or means must be found for deciding on independent grounds to which class we are to refer the instance. If, for example, it

were known that every country produces its own rain, we should choose the third rule, for it would be a case of drawing from a limited supply. If again we had reasons to believe that the rain for our country might be produced anywhere on the globe, we should probably conclude that the past rainfall threw no light whatever on the prospect of a continuance of wet weather, and therefore take the second. Or if, finally, we knew that rain came in long spells or seasons, as in the tropics, to have had ten wet days in succession would make us believe that we had entered on one of these seasons, and that therefore the next day would probably resemble the preceding ten.

Since then all these forms of such an Inductive rule are possible, and we have often no *à priori* grounds for preferring one to another, it would seem to be unreasonable to attempt to establish any universal formula of anticipation. All that we can do is to ascertain what are the circumstances under which one or other of these rules is, as a matter of fact, found to be applicable, and to make use of it under those circumstances.

CHAPTER XIII.

ON THE CREDIBILITY OF EXTRAORDINARY STORIES.

§ 1. It is now time to recur for fuller investigation to an enquiry which has been already briefly touched upon more than once; I mean the validity of testimony to establish, as it would be said, an otherwise improbable story. It will be remembered that in a previous chapter (the fifth) we devoted some examination to an assertion of Bishop Butler, which seemed to be to some extent countenanced by Mr Mill, that a great improbability before the event might become but a very small improbability after the event. In opposition to this I endeavoured to show that the different estimate which we undoubtedly formed of the credibility of the examples adduced, had nothing to do with the fact of the event being past or future, but arose from a very different cause; that the conception of the event which we entertain at the moment (which is all that is then and there actually present to us, and as to the correctness of which as an adequate representation of facts we have to make up our minds) comes before us in very different ways. In one case it was a mere guess of

our own which we knew from statistics would be right in a certain proportion of cases; in the other it was the assertion of a witness, and therefore the appeal was not now to statistics of the event, but to the trustworthiness of the witness. The conception, or 'event' if we will so term it, had in fact passed out of the category of guesses (on statistical grounds) into that of assertions (most likely resting on some specific evidence), and would therefore be naturally regarded in a very different light.

§ 2. But it may seem as if this principle would lead us to far more startling conclusions than any which we reject. For, by transferring the appeal from the frequency with which the event occurs to the trustworthiness of the witness who makes the assertion, is it not implied that the probability or improbability of an assertion depends solely upon the veracity of the witness? If so, ought not any story whatever to be believed when it is asserted by a truthful person?

Undoubtedly it ought, on the data now before us. But let it be clearly understood what conditions are implied in this limitation. Only under the two following conditions is it true that the credit we give to the statement of a witness is entirely independent of anything in the nature of the event to which he testifies. In the first place, the question must be really one of Probability; that is, the asserted event

must be only rare, or in other words, must be admitted actually to happen sometimes, though we may not know the frequency of its occurrence. In the second place we must adhere strictly to our data, and judge of the trustworthiness of the witness in any particular case simply from the statistical frequency with which he tells truth and falsehood.

We will now briefly examine the meaning of these conditions, and ascertain the consequences when they are not adhered to. We shall thus be able to clear up a great deal of confusion which seems to exist upon this subject. It will also appear how very narrow is the province of pure Probability, and how arbitrary therefore are the restrictions which have to be introduced if we will insist upon judging of ordinary events by its rules.

§ 3. We will begin with the second of the above-mentioned conditions. In judging of the probability of any assertion of a witness of *given veracity, it is

* I have already (Chap. x.) given reasons against the propriety of applying the rules of Probability with any strictness to such examples as these. But, although all approach to numerical accuracy is unattainable, we do undoubtedly recognize a distinction in ordinary life between the credibility of one witness and another; such a rough practical distinction as this will be quite sufficient for the purposes of this Chapter. For convenience, and to illustrate the theory, the examples will mostly be stated in a numerical form, but it is not intended to be implied that any such accuracy is really attainable in practice.

implied that our *sole* ground for attributing truth to the story is that a given proportion of the assertions made by that witness are found to be true. In other words, it is implied that the particular inference, namely the truth of the statement in question, is simply a deduction from the general proposition, namely, the proportion of the statements of the witness which are true.

§ 4. The meaning and propriety of such an implication will appear more clearly if we examine what follows from neglecting or denying it. This will be best introduced by the examination of a phrase which is often employed in these discussions, and which the reader must have frequently met, I mean 'a balance or contest of opposite improbabilities.' It was Hume, I believe, in his Essay on Miracles, who first brought the expression into general use, but it has been adopted by Paley and by most of the other opponents of Hume, and has met with very general acceptance. What is meant by such a phrase, I apprehend, is this;—that in forming a judgment upon the truth of any assertion, we find that the assertion is comprised in two or more different classes, and that according as we referred it to one or other of these different classes our judgment as to its truth would be different. In other words, there are two or more different sources of evidence, any one of which, if we adhered to that alone, would lead us to a conclusion at variance with

that which is supported by the others. It seems to be assumed in the phrase, 'contest of opposite improbabilities,' that, when these different sources of evidence coexist together, they would all in some way retain their probative force so as to produce a contest, ending generally in a victory to one or other of them. I should say, on the contrary, that, in the case under discussion, if we adhere to our data one of these sources of evidence simply and entirely supersedes the other; if we do not adhere to the data there is a contest, no doubt, but it is one for the decision of which no rule can be given, and the result of which is therefore entirely arbitrary.

§ 5. We adhere to our data if we judge of the truth of the individual assertion entirely from the frequency with which the witness speaks truth and falsehood. If so, of course, one of the sources of evidence entirely supersedes the other. It is clearly intended when the data are given to us that this should be the case. For by the improbability of the event is meant nothing else than the improbability of our guessing it, owing to the rarity of its occurrence. This is one possible ground on which our opinion might have been rested. But the witness is not supposed to have guessed, or, if he did guess, this, like any other source of error on his part, is already included in the figure which describes his veracity. This is quite a different ground on which to rest our

conviction, and when this is given it clearly supersedes the other. Any datum therefore which involves the veracity of a witness carries its paramount authority on its face, for when this is given all necessity for us to guess is removed.

§ 6. The course described in the last section is the one which ought to be taken, and in that case all 'contest' is evaded. But when a contest is provoked the result is very different. Suppose that a witness who lies once in ten times tells a very extraordinary story. When the statement is made our datum is, as before, This is a statement made by a witness who lies but once in ten times; and from this only ought we to draw our inference. But practically we seldom submit to this restraint. Some such reflection as the following will almost unavoidably arise;—This statement is of a suspicious kind, that is, it is of a kind which, when made by people generally, is in most cases found to be false. Now if we take these extraneous considerations into account, what conclusions are we to draw? Here there is a real contest of the kind already discussed in the chapter on Induction (chap. VII. § 15), and I do not see how any possible way of deciding the contest can be found, except by introducing an arbitrary supposition, or by adding to our information by a fresh appeal to specific experience.

§ 7. Our perplexity is as follows;—We have

before us a statement. On this occasion it is made by a witness who lies in the long run but once in ten times; it is however a statement of a kind which is generally false; stated numerically it is found (let us suppose), when we examine the case of many different witnesses, to be false ninety-nine times in a hundred. We are here brought to a dead lock, our science offering no principle by which we can form an opinion or attempt to decide the question. It was shown in the chapter on Induction, already referred to, that an indefinite number of conclusions were all equally possible. For example, all the witness's extraordinary assertions might be true, or they might all be false, or they might be true and false in almost any proportion whatever. This being the case, how can any definite rule be obtained for the solution of the difficulty? The issue of the contest when, as in this case, there is a contest, seems hopelessly indeterminate.

§ 8. I am quite aware that some solutions have been offered. Hume speaks of our *deducting* one probability from the other and apportioning our belief to the remainder. Doubtless he would have laid no stress upon the numerical accuracy of the process, but even this semblance of accuracy must be abandoned. Archbishop Thomson considers that one probability entirely *supersedes* the other. Were he confining his remarks to the class of instances at present under notice, this would be correct, but a reference to his

remarks in the *Laws of Thought* will show, I think, that this is not the case.

There is one conclusive objection to all such solutions of the difficulty; they are all attempts to decide *à priori* what can only be decided by a specific appeal to experience. It has been maintained throughout, in this Essay, that any rule for apportioning the amount of our belief must, if the rule claims to be correct, contain a correct assertion about the statistical frequency of events. If therefore it can be shown (as I shall now attempt to show) that the events in question are perfectly indeterminate, such a rule is at once condemned.

§ 9. The way in which the error creeps in is as follows;—The witness is said to report an 'improbable event;' it is inferred therefore that his veracity must in that case be more questionable. I dislike speaking of an improbable event in Probability, though one is often obliged to do so, but let it be clearly understood what is meant by the term. It denotes simply an event which does happen, but happens rarely, and of the existence of which therefore we should, judging only by statistics, be extremely doubtful. But why should a witness be at all less likely to tell the truth when relating a rare event than when he is relating a common event? It may be that he is, but it is also possible that the reverse should be the case; if so, a moment's consideration

will show that it is only by the supposed effect of the rarity of the event upon the veracity of the witness that his story becomes more or less probable.

§ 10. For suppose that the fact of a witness describing a very uncommon event makes him more than usually careful, and therefore actually adds to his veracity. We should in that case receive his extraordinary assertions with even more readiness than his ordinary assertions. This is in reality no such unlikely supposition. Let us assume that a man of ordinary intelligence and a philosopher—say Professor Owen,—make some assertion about common things; we believe them both. Let them now each describe some extraordinary *lusus naturæ* or monstrosity which they report that they have seen. I presume that almost every one would believe the assertion of the Professor nearly as readily in the latter case as in the former, whereas when the same story came from the uninstructed man it would be received with great hesitation. Whence arises the difference? From the assumption that the philosopher's assertions will be to the full as accurate in matters of this kind as in those of the most ordinary description, whilst in the case of the other man we are far from feeling this confidence. Even if the reader is not prepared to go this length he will allow, I presume, that the difference of credit which he would attach to the two assertions, when coming from the philosopher, would be very much less

than what it would be when they came from other men; the admission of any such difference, no matter to what extent, is an admission of the principle here contended for.

§ 11. Instances of the kind just mentioned are of course exceptional; as a general rule the known comparative rarity of the event asserted does add to the improbability of the assertion. It may add to it to any amount whatever. There are many forms of lying gossip about the falsehood of which we feel so certain, that we should hardly believe them though coming from the mouth of the most trustworthy of men. Whilst therefore it is quite true, in certain instances, that the more uncommon the event the more improbable does the testimony of the witness become who asserts it, it is also true in other instances that the direct reverse is the case. Sometimes the rarity of the event may make the testimony actually more trustworthy; sometimes it may, though taken into account, leave the testimony unaltered. We do not argue directly from the rarity of the event; our belief, though undoubtedly often influenced by the rarity, is only influenced by the supposed effect which that may have upon the veracity of the witness.

I consider therefore that, on the rules of Probability, whenever a story of any kind whatever is described as coming from a witness of a given degree of veracity, our only course is to accept the story as having that

given degree of probability in its favour *. If we do not do this we fall into utter confusion. All attempts to allow for the alteration of the witness's veracity according to the description of story which he tells must, from the nature of the case, be entirely arbitrary or depend upon the sagacity of the observer,—no fixed rules can help us.

§ 12. Hitherto we have proceeded on the assumption that the question is properly one of Probability. In other words, we have assumed it to be admitted that the event asserted to have happened does or will really happen, the only claim it has to be called improbable being that owing to its rarity we should be very little inclined to expect its occurrence in any particular instance. But such an assumption cannot long be practically adhered to, for the term 'improbable,' when applied to an event, has a far wider signification in popular estimation. We will therefore make a brief enquiry into what follows when this assumption is denied. It is the more necessary to do so, because it is this aspect of the question which possesses most popular interest, especially in reference to the credibility of testimony when applied to miraculous stories.

§ 13. The best way of approaching the subject will be by examining two or three examples in suc-

* See the note at the end of this Chapter.

cession. We shall then perceive that there are two distinct principles at work, upon one of which the science of Probability is competent to speak, whilst upon the other it is not.

(1) A witness tells me that he has thrown sixes five times running. This is a question of pure Probability; on the grounds already so fully discussed, we form our opinion solely from the degree of trustworthiness of the witness.

(2) Again, he tells me that he has seen a sheep with five legs. The event which he reports is known to happen sometimes, perhaps not more unfrequently than the throw of the dice in the last example. If the question is proposed to us as one of pure Probability, and if the credibility of the witness is our only datum, we judge, as in the last example, solely by this credibility.

Or, if we do not confine ourselves to our data, we might bring the assertion under the category of 'extraordinary stories.' We might consider that, from its partaking of the marvellous, the story is one of a kind commonly found to be erroneous, and therefore change, in some arbitrary proportion, the degree of credibility we assign to the witness.

(3) Again, he says that he has seen a sheep with *ten* legs. We feel here that we are getting on to different ground. The practical indisposition to confine ourselves to our data, which was considerable

in the last example, is now uncontrollable. The existence of such a monster, as that described, is doubted; this doubt, which will intrude itself into the question, can only be settled, if it is to be noticed at all, by Inductive principles and skill in applying them to natural science. Give what weight we will to the unwarranted alternative suggested in the last example—in other words, alter as we will the figure of the witness's veracity, owing to the fact of his story being extraordinary—it will fail to satisfy. The doubt we feel is far too serious for its force to be spent by a mere arbitrary correction of the veracity.

§ 14. It is owing in part to considerations such as these that we are forced, as it appears to me, to take the view of Probability adopted in this work. I see no other satisfactory way than to separate the province of Probability distinctly from that of Induction, to assign to the latter the task of preparing the statistics, vouching for their truth, and extending them as far as admissible, and to relegate to the former the far narrower task of finding rules for inferring the particular and individual cases from these extended statistics.

It may perhaps be urged in reply, that the case of this monstrosity does not differ essentially from many examples that we meet with in pure Probability. It is true (it may be said) that such a sheep has not yet

been seen, but after all it may be like some rare collocation of dice or balls which will come at last in its turn, and is not therefore more unlikely than any other particular event of the kind. True, it may; and also it may not; and the existence of this possible alternative is what necessitates the adoption of further evidence. To argue that because it is not more difficult to believe in a throw that has not been hitherto experienced than in one that has, that therefore it is equally easy to believe in any *event* hitherto unexperienced, is to lose sight of the foundations of the science. What we reason from in the case of the balls is not the limited series which has actually occurred, but the infinite series which analogy and Induction lead us to adopt. As shown in Chapter III, we calculate from a substituted series, not from the fragment given to us; from potential, therefore, not from actual experience. In the case of the balls or dice this substitution is so natural that its validity is never doubted; moreover, there is a tolerably complete unanimity as to the terms which the succession of throws can be expected from time to time to produce. But in the case of the monstrosity it is very seriously doubted whether such a term is in the series at all. To say that such a sheep may yet be produced, if we wait long enough, is to assert what is possible, but more than that is wanted. Before we can calculate the chance of the monstrosity by our own guessing, we

must know in what proportion of cases things of its kind occur. Before we can do so from the witness's assertion, we must at least be satisfied that such creatures are possible, and that the witness has no bias towards lying about them; then the knowledge of their frequency, as already described, would not be needed. But, till these prior questions are settled, nothing can be inferred. For the solution of them Probability must invoke the aid of Observation and Induction, and pause till they have pronounced.

§ 15. It appears therefore to me that the attempts so often made to apply the theory of Probability, by means of the credibility of testimony, to the establishment or otherwise of miraculous stories, involve some confusion and ambiguity in the meaning of the word 'improbable.' As I have already so frequently said, an improbable event in our science is one which is admitted to occur sometimes, but whose occurrence we should not have anticipated, owing to its rarity; popularly it means an event whose occurrence is doubtful, and is perhaps the very point in dispute.

§ 16. The foregoing results may be summarized as follows;—To have it given, as our only datum, that a certain story is asserted by a witness of given veracity, binds us down to judging of its truth, whatever may be the nature of the story, solely by the relative frequency with which the witness speaks

truth and falsehood. Practically, however, we generally transgress our data, in which case the problem assumes one of two forms. It may remain one of Probability, by our admitting that the event asserted does sometimes happen, but considering that as an 'extraordinary' event it is not to be judged by the average veracity of the witness; then we should generally (but not always) diminish the probability of his veracity, but the amount of this diminution is entirely arbitrary. Or the problem may cease to be one of Probability by our *not* admitting that the event does or may occasionally happen; then the question must be resigned to the far wider science of Induction and of evidence generally.

§ 17. A few words may be added here about the combination of testimony, though no new principles seem to be required for the discussion of this subject. Suppose that two witnesses whose veracity is respectively $\frac{9}{10}$ and $\frac{11}{12}$ combine in asserting the same story; what strength should this give as to the truth of the story? This question seems to me to fall under the second of the rules of inference discussed in the fourth chapter. The chance of a lie from the witness A is $\frac{1}{10}$; the chance of a lie from B is $\frac{1}{12}$; therefore the chance of a combination of lies from the two is $\frac{1}{120}$. Now, since both A and B agree in making the same statement, their statement must be true unless they are both lying. The chance therefore that the event

has not happened is $\frac{1}{120}$, or the odds in its favour are as 119 to 1.

Practically this combination of testimony generally meets us as a modification of that aspect of the question described in the sixth section. A witness, *A*, makes a statement, but it strikes us as being a statement of an extraordinary or suspicious character. Hence we very much diminish our estimate of his veracity for questions of that kind. Then *B* comes forward and confirms the story; we correct his figure of veracity in the same way. But the combination of the two statements so greatly increases the probability of the truth of the thing stated, as to outweigh in many cases the separate diminution of their values. The product of two factors, after each has been diminished, may be greater than either was singly before. Similarly if there be more than two witnesses.

The main practical objection against the value of such conclusions as these is the extreme difficulty, in fact the impossibility in many cases, of being certain that the witnesses are independent. The more nearly the stories resemble one another the nearer are we to the real combination of testimony in question, and therefore the greater should be our confidence in the truth of what is asserted. But these are precisely the cases in which our doubt will be greatest as to the witnesses being independent. The nature of the

subject seems to forbid any such strict rules as are demanded in Logic.

§ 18. It appears to me therefore that the science of Probability has really little or no direct bearing upon the credibility of miracles, that is if we limit the applicability of the science to that class of enquiries to which it seems most properly to belong. Their credibility must be established on some independent ground before we can judge of the validity of testimony to support them. Let it be shown that they are not intrinsically incredible, and a combination of testimony might establish them as it might establish anything else. If we do not take this course, the way in which the mere force of testimony might be evaded is obvious from what has been already said. We show that ten persons agree in supporting a miraculous story; but the opponent cannot be prevented from merely putting his arbitrary alteration of the veracity of the witnesses at such a point as to make the testimony of ten persons when reporting such a thing of very little value.

The question of miracles has however been so incessantly introduced in reference to the credibility of testimony and thence to the Science of Probability, that I cannot forbear devoting a few pages to the subject; though, as mentioned above, I conceive the connection between miracles and Probability to be only indirect.

§ 19. A necessary preliminary will be to decide

upon some definition of a miracle. It will, I apprehend, be admitted by most persons that in calling a miracle 'a suspension of a law of causation,' we are giving what, though it may not amount to an adequate definition, is at least true as a description. It is true, though it may not be the whole truth. It is this aspect moreover of the miracle which is now exposed to the whole brunt of the attack, and in support of which therefore the defence has generally been carried on. For these reasons we will commence with this view of it.

Now it is obvious that this, like most other definitions or descriptions, makes some assumption as to matters of fact, and involves something of a theory. The assumption clearly is, that laws of causation prevail universally, or almost universally, throughout nature, so that infractions of them are marked and exceptional. This assumption is made, but I do not think that anything more than this is necessarily required. I mean that there is nothing which need necessarily restrict us to one or other of the two principal schools which are divided as to the nature of these laws of causation. The definition will serve equally well whether we understand by *law* nothing more than uniformity of antecedent and consequent, or whether we assert that there is some deeper and more mysterious tie between the events than mere sequence. The term 'causation' in this sense is com-

mon to both schools, though the one might consider it inadequate; we may speak, therefore, of 'suspensions of causation' without committing ourselves to either.

§ 20. It should be observed that the aspect of the question suggested by this definition is one from which we can hardly escape. Attempts indeed have been sometimes made to avoid the necessity of any assumption as to the universal prevalence of law and order in nature, by defining a miracle from a different point of view. A miracle has been called, for instance, 'an immediate exertion of creative power,' 'a sign of a revelation,' or, still more vaguely, an 'extraordinary event.' But nothing would be gained by adopting any such definitions as these. However they might satisfy the theologian, the student of physical science would not rest content with them for a moment. He would at once assert his own belief, and that of all other scientific men, in the existence of universal law, and enquire what was the connection of our definition with this doctrine. An answer would imperatively be demanded to the question, Does the miracle, as you have described it, imply an infraction of one of these laws, or does it not? And an answer must be given, unless indeed we reject his assumption by denying our belief in the existence of this universal law, in which case of course we put ourselves out of the pale of argument with him. The necessity of having to recognize this fact is growing upon men day by day,

with the increased study of physical science. And since this aspect of the question has to be met some time or other it is as well to place it in the front. The difficulty, in its scientific form, is of course a modern one, for the doctrine out of which it arises is modern. But it is only one instance, out of many that might be mentioned, in which the growth of some philosophical conception has gradually affected, and at last shifted the battle-ground, in some discussion with which it might not at first have appeared to have any connection whatever.

§ 21. So far our path is plain. Up to this point disciples of very different schools may advance together; for in laying down the above doctrine we have carefully abstained from implying or admitting that it contains the whole truth. But from this point two paths branch out before us, paths as different from each other in their character, origin, and direction, as can well be conceived. As this chapter is only a digression, I will confine myself to stating briefly what seem to be the characteristics of each, without attempting to give the arguments which might be used in their support.

(I.) On the one hand, we may assume that this principle of causation is the ultimate one. By so describing it, we do not mean that it is one from which we consciously start in our investigations, as we do from the axioms of geometry, but rather that it is the

final result towards which we find ourselves drawn by a study of nature. Finding that, throughout the scope of our enquiries, event follows event in never-failing uniformity, and finding moreover (some might add) that this experience is supported by a tendency or law of our nature (it does not matter here how we describe it), we may come to regard this as the one great principle on which all our enquiries should rest.

(II.) Or, on the other hand, we may admit a class of principles of a very different kind. Allowing that there is this uniformity so far as our experience extends, we may yet admit what I can see no other way of describing than by calling it a Superintending Providence. To adopt an aptly chosen distinction of Professor Kingsley's, it is not to be understood as *over-ruling* events, but rather as *underlying* them. I repeat again, that it is not my present object to enter into any discussion as to whence we obtain this idea.

§ 22. I cannot see how any reflecting mind can fail, at the present time, to be subject to one or other of these prepossessions. This word prepossession has, in common use, a bad signification almost equivalent to prejudice, but I use it here because I know no other which would express my meaning equally well. If the principles by which we judge were intuitively and immediately obvious, we might conceive any one to be able in a sort of way to divest him-

self of prepossessions. These principles could be stated, like the axioms on the first pages of Euclid, and any casual bystander would be able to state in two seconds whether he adopted them or not. This gives the appearance of commencing absolutely *ab origine*. But if first principles are only to be acquired by long and laborious reflection, such a demand as this is quite out of the question. To require a disputant to strip himself of his axioms, flourish them in his opponent's face, and then deliberately put them on again, is to require him to appeal from his mind when mature to what it was when immature. Though acquired in the most unexceptionable way such principles may, in reference to any particular problem, be called prepossessions; and if they are challenged or denied, all that any one can do is to repeat his conviction of their truth, and his belief that other persons by reflection will come to regard them as he does; he cannot compel the assent of his opponent.

§ 23. Now it is quite clear that according as we come to the discussion of any particular miracle or extraordinary story under one or other of these prepossessions, the question of its credibility will assume a very different aspect. It has been strangely overlooked, in many recent discussions, that although a difference about *facts* is one of the conditions of a *bonâ fide* argument, a difference which reaches to ultimate principles is fatal to all argument. The

possibility of present conflict is banished in such a case as absolutely as that of future concord. A large amount of recent literature on the subject of miracles seems to labour under this hopeless defect. Arguments have been brought for and against the credibility of stories without the disputants (on one side at least) appearing to have any adequate conception of the chasm which separated one side from the other.

§ 24. The following illustration may serve in some degree to show the sort of inconsistency of which I am speaking. A sailor reports that in some remote coral island of the Pacific, on which he had landed by himself, he had found a number of stones on the beach disposed in the exact form of a cross. Now if we conceive a debate to arise about the truth of his story in which it is attempted to decide the matter simply by considerations about the validity of testimony, without introducing the question of the existence of inhabitants, we shall have some notion of the unsatisfactory nature of many of the current arguments about miracles. All illustrations of this subject are imperfect, but a case like this, in which a supposed trace of human agency is detected interfering with the orderly sequence of other natural causes, is as much to the point as any illustration can be. The thing omitted here from the discussion is clearly the one important thing. If we suppose that there is no inhabitant, we shall probably disbelieve the story, or

consider it to be exaggerated. If we suppose that there are inhabitants, the question is at once resolved into a far higher one. The credibility of the witness is not the only element, but we should take into consideration the character of the supposed inhabitant, and the object of such an action on his part.

§ 25. I am aware that considerations of this character are often introduced into the discussion, but it appears to me that they are introduced to a very inadequate extent. It is often urged, after Paley, "Once believe in a God, and miracles are not incredible." Such an admission demands some extension. It should rather be stated thus, Believe in a God whose working may be traced throughout the whole moral and physical world. It amounts, in fact, to this;—Admit that there is a *design* which we can trace somehow or other in the course of things; admit that we are not wholly confined to tracing their connexion, or following out their effects, but that we can form some idea, feeble and imperfect though it be, of a *scheme* *. Paley's advice sounds too much like saying, Admit that there are fairies, and we can account for our cups being cracked. The admission is not to be made in so off hand a manner. To any one labouring under the difficulty we are speaking of, this simple belief in a God almost out of relation to nature,

* The stress which Butler lays upon this notion of a scheme is, I think, one great merit of his *Analogy*.

whom we then imagine to manifest himself in a perhaps irregular manner, is altogether impossible. The only form under which belief in the Deity can gain entrance into his mind is as the controlling Spirit of an infinite and orderly system. In fact it appears to me, that it might even be more easy for a person thoroughly imbued with the spirit of Inductive science, though an atheist, to believe in a miracle which formed a part of a vast dispensation, as the Christian miracles do, than for such a person, as a theist, to accept an isolated miracle. I repeat again, that I am not concerned at present with the origin of our belief in design or a Providential scheme. If any can find it in a study of physical science so much the better for them; but whether it be obtained thence, or from moral and metaphysical grounds, or from Revelation, we must possess it, or miracles at the present day will be hard of credit.

§ 26. It is therefore with great prudence that Hume, and others after him, have practically insisted on commencing with a discussion of the credibility of the single miracle, treating the question as though the Christian Revelation could be adequately regarded as a succession of such events. As well might one consider the living body to be represented by the aggregate of the limbs which compose it. What I complain of in so many popular discussions on the subject is the entire absence of any recognition of the different

ground on which the attackers and defenders of miracles are really standing. Proofs and illustrations are produced in endless variety, which involving, as they almost all do in the mind of one at least of the disputants, the very principle of causation the absence of which in the case in question they are intended to establish, they fail in the one essential point. To attempt to induce any one to disbelieve in the existence of physical causation, in a given instance, by means of illustrations which to him seem only additional examples of the principle in question, is like trying to stop the flow of a river by shovelling in snow. Such illustrations are plentiful in times of controversy, but being in reality only modified forms of that which they are applied to counteract, they change their shape at their first contact with the disbeliever's mind, and only help to swell the flood which they were intended to check.

§ 27. The bearing of the last few sections may be expressed as follows. Any one who believes that the moral and physical world form one great scheme need find no insuperable difficulty in accepting a Revelation which forms a portion of such a scheme, nor consequently in accepting the miracles, collectively and individually, which are connected with the Revelation. But if, on the other hand, we start with the Inductive principle of uniform causation, and then attempt (leaving the notion of Providential superintendence

out of sight) to establish, first, such and such a miracle, and thence a Revelation; it is hard to see how, on such principles, in the present state of feeling about scientific evidence, any accumulation of testimony could do more than baffle and perplex the judgment at the time, and leave us finally in doubt.

§ 28. In this brief digression I have been able to do little more than state my own opinion, and the principal grounds on which it rests. Direct argument against those who should differ from me, I could scarcely, for the reasons already given, profess to offer. And here the matter must be left; for where men differ on such fundamental points speedy agreement is hopeless. A difference as to facts may be set at rest sometimes in a few minutes. But when the difference which separates party from party is one of ultimate principles, approximation to one another may be indefinitely delayed. The suffrages of ages are required on matters of this kind before a final judgment is obtained.

In the remarks in § 11 it is implied that whatever may be the number of possible contingencies in any given case, the amount of our belief in the assertion is the same, being that fraction of certainty (to use the common phrase) which is assigned by the figure which denotes the witness's veracity. If this be $\frac{9}{10}$, then, whether he say that a penny has given head, or that No. 79 has been drawn from a lottery of 1000 tickets, or make any other assertion whatever, we say that the chances are $\frac{9}{10}$ that the assertion is true. I am aware that the ordinary view is

somewhat at variance with this, but the variation is produced by what seems to me a rather arbitrary assumption. It arises in the following way: One in ten of the assertions of the witness are false; but what will be the nature of these false assertions? I have assumed that we can tell nothing about their nature, so that when the witness does not tell the truth he may say anything. A common assumption, on the contrary, is that his false assertions must be confined in the above examples to telling numbers and throws, but telling these falsely. Hence, on my hypothesis, although some of the lies may undoubtedly take the form of asserting the number in question when it did not occur, yet these will, on the average, be quite inappreciable in number, owing to the indefinite scope which the witness has for lying in other directions. On the other hypothesis, however, the number of these occasions on which an event is asserted without having happened will be very important. Out of 9990 occasions upon which any given number, say 65, is drawn, the witness will lie 999 times. On my hypothesis no finite proportion of these lies will take the form of asserting 79; their effect is lost by their radiating out freely, as one may say, into space, and so being dissipated. If, on the other hand, we suppose them to be confined to the limits imposed by the numbers in the lottery, their effect will be concentrated within these limits. On this view one out of these 999 particular lies will be a false assertion of the drawing of No. 79. And the same will apply when any of the other numbers are drawn. On this view therefore (unlike the one I have adopted) 79 will be announced when it did not occur in a certain regular proportion of cases in the long run. The assumption thus adopted seems to me arbitrary, but I do not think that the question is of any great practical importance. A brief discussion of some complications which are thus introduced will be found in Mill's *Logic*, Book III. ch. 25. § 6.

CHAPTER XIV.

CAUSATION.

§ 1. In several of the foregoing chapters we have been obliged to touch incidentally upon some of the philosophical and religious disputes into which the Science of Probability has at different times got itself embroiled. The interest and importance of these disputes however is so great that we must now enter into a more explicit and detailed examination of some of them. They almost all arise from the same source,—the bearing upon the doctrine of Universal causation of the assumption with which we started, and which was so fully discussed in our first two chapters. This assumption, the reader will remember, was that of a series as to the details of which we were in ignorance, whilst we possessed some knowledge about the average of the individuals of which the series was composed. The exact meaning of this assumption was, I hope, assigned with sufficient precision; but as the doctrine of Universal Causation, which also enters into the conflict, has many different meanings, it will be necessary to determine accurately which of them is to be adopted here.

§ 2. I wish it to be understood then that we are about to enter into no metaphysical or ontological discussions; no enquiries will be made as to the intimate nature of causation, if it have any, nor need any of the associations excited by the term 'efficient cause' be introduced. We will employ the word simply in the sense which is becoming almost universally adopted by scientific men, viz. that of invariable unconditional sequence.

It is in this sense that the word *cause* is used by Mr Mill. I refer to him in particular because his works contain one of the fullest and clearest explanations of the term with which we are now concerned. He points out indeed that by the 'antecedent' must be understood the 'sum total of antecedents,' explaining his meaning as follows;—"It is seldom, if ever, between a consequent and a single antecedent that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents; the concurrence of all of them being necessary to produce, that is, to be certain of being followed by, the consequent. In such cases it is very common to single out one only of the antecedents under the denomination of cause, calling the others merely conditions." Several pages of explanation follow, devoted to tracing out the arbitrary and unphilosophical nature of any such distinction as that mentioned in the last clause, and to enforcing the fact that throughout his work he shall

understand by cause "the sum total of the conditions, positive and negative, taken together; the whole of the contingencies of every description which being realized the consequent invariably follows." (Mill's *Logic*, Bk. III. ch. v. § 3.)

This meaning of the term is rapidly becoming the popular, or rather, the popular scientific one. There will of course be wide differences between different persons as to the extent over which they believe that causation in this sense can be predicated. All I ask is, that there may be no confusion; taking the above meaning of the word, let each assign its range of application according to his own opinion, but let us at least agree about its meaning. In logical language, having fixed the connotation of the word it must be left to the reader to assign the denotation.

§ 3. Our first task will be to analyse the meaning of the term; in doing so we shall find it, I think, neither so generally applicable nor so consistently applied as many persons seem to suppose. The two principal points to be discussed in the definition adopted above seem to me to be the following;—(1) The introduction of the *sum-total* of the antecedents into the cause; (2) the regarding the cause as the *immediate* antecedent. This latter condition is often not so explicitly stated, but we shall easily see that it is implicitly involved.

§ 4. The first departure from the definition is in

adopting the common practice of omitting some of the elements which conjointly form the invariable antecedent. This omission is almost forced upon us whenever we wish to make any use of the law of causation; for as the cause was defined it appears to be barren and impracticable. It is defined in a hypothetical form, and the hypothesis is one that in most cases may not be, and in some certainly is not, realised. The cause is defined to be such a collection of antecedents as *if* it were repeated the consequent would again follow. But *will* it be repeated? very seldom, perhaps never, if we insist on having all the antecedents accurately introduced. In very simple examples we do frequently find repetition, or something undistinguishably resembling it, but the moment we examine cases of any degree of complexity, though there may be repetition of many of the separate elements, the precise combination is generally unique. This would be the case, for instance, with any thing which affected our bodies; for no man's constitution resembles exactly that of any one else. Still more would it be so if we were examining any of the greater operations of nature, such as thunderstorms, rain, Aurora Borealis.

In these cases it would be true practically, and in almost all cases true theoretically, that the same antecedents never do actually recur. If so what becomes of a definition which involves this hypothetical

repetition? It may stand as an expression of belief of what would occur in contingent and possible circumstances; but far more than this is requisite if any work is to be got out of the definition. It would be almost like defining a mortal as one who *if* he lived to be 150 would be in the last stage of decay. Such a definition as this is clearly barren and useless. In fact we seem to be reduced to the following dilemma;— If we adhere rigidly to the sense of the term which was laid down at first no use can be made of it, for repetition then is certainly rare, and perhaps is not to be looked for at all. If we are to make any use of the formula we are forced to omit some of the antecedents, and then it ceases to be conformable to fact. It appears that we are driven to make our election between the useless and the false.

§ 5. The reply would probably be that a sufficiently close approximation to real repetition to avoid all important error may frequently be found. This is certainly undeniable. It is obvious to all that events very similar to one another in most of their characteristics do constantly repeat themselves, and from this circumstance an abundance of inferences of great practical value may be obtained. But this is falling short of the requirements of a Logic of Induction. What we are at present concerned with is, not the looser form of the doctrine of Causation as it is practically made use of, but the strict form in which

it appears as the basis of a science of inference about external things.

§ 6. After a statement of the relation of cause and effect described above, we are often reminded that there is another relation in which events may stand to one another, one dependent indeed on causation, but in which the antecedent, not being the *immediate* antecedent, cannot be considered the cause. The antecedent and consequent are, if one may so express it, not in contact in this case, but are a little removed from one another. The sequence is often a tolerably regular one, so as to present a certain degree of uniformity, but not being an immediate sequence, it may generally be ultimately resolved into cases of causation by the discovery of intermediate links. These uniformities are known by various names, such as, Empirical laws, or Uniformities dependent on causation. This distinction has been brought into notice in such recent disputes as those raised by the works of Mr Buckle and others of the same school. When any uniformity has been observed in the conduct of men, we have been reminded that a mere generalization is a very different thing from the sequence of cause and effect. Mr Mill throughout his treatise seems to lay considerable stress upon this distinction; he differs indeed from Dr Whewell in the language employed to describe the distinction, but nevertheless speaks of it as "one of the most fundamental distinctions in science;

indeed it is on this alone that the possibility rests of framing a rigorous canon of Induction."

§ 7. I cannot help thinking that this distinction, though having a foundation in nature, is, in the explicit form in which it is stated above, nothing but an example of the *idolum fori*, arising from the necessities of common language, and deriving its principal support from common illustrations. Substitute for the time-honoured 'chain of causation,' so often introduced into discussions upon this subject, the phrase a 'rope of causation,' and see what a very different aspect the question will wear. For what is the conception necessarily conveyed by a chain? that of determinate distinct links following one another in succession, of stages marked off from one another in nature as well as distinguished in language. Whereas what we really find in nature is an evolution rather than a succession; the stages when examined at a little distance from one another are tolerably distinct, but, when closely examined, each merges into the next and blends with it by insensible degrees. They are like the strands of a rope; at a little distance there may be what one might call successive patches or stages, but when we look closer we find that each strand continues without the slightest break of continuity. Instead of having links definitely marked out for us, the steps and stages have to be assigned by ourselves, and have a great deal of what is arbitrary in them.

§ 8. This mistake finds much countenance in the common practice of using letters of the alphabet to denote the causes and effects. The following is a fair sample of this mode of illustration. 'Within the limits of past experience A has been always followed by B, A therefore is probably the cause of B; still there may be some intermediate link C, between A and B, which is respectively effect of A and cause of B. Or even eventually a D may be discovered between C and B, and so on.' I do not, of course, give the above as a sample of the reasoning employed, but simply of the phraseology in use. It involves throughout the conception of successive links of a chain, the only doubt ever felt being that a step or two may have been overlooked, though, if so, these steps also will be links. But is this conception consistent with fact? Surely it needs but a very little reflection to perceive that all these stages, which we thus mark out, exist as distinct stages only in our classification, and that when we look at nature we find, not an A and B as successive links with a possible intruder C between them, but rather an A and B as successive portions of a strand between which, if we had chosen, we might have interpolated an indefinite number more. A dose of arsenic will cause death, but how many intermediate stages are there between these? Even where the cause and effect seem most proximate, as for instance in an explosion giving rise to the sensa-

tion of sound, we might if we pleased interpolate any quantity more of what are called links.

§ 9. All this is too obvious to have escaped notice, but its bearing on the distinction between laws of causation and empirical laws has been generally neglected. For if the above remarks be true, this distinction vanishes; it vanishes, at least, as an accurate theoretical distinction, though it may be retained in a looser form for practical purposes. It is essential to the existence of a cause, as an unconditional invariable antecedent, that it should immediately precede the effect; if there is any interval we can never insure the succession from being frustrated by the intrusion of some counteracting agency. I am aware that such counteracting agency is often expressly excluded, but I cannot help regarding this exclusion as being entirely unwarranted. By what right is the exclusion maintained, when the object is to prop up and insure from failure a law of causation, but refused when wanted to perform the same service for an empirical law? Only exclude counteracting agencies and any observed empirical uniformity will be as 'unconditional' and invariable as can be desired; suffer these agencies to enter and the laws of causation will cease to possess these characteristics. If we admit any interval between the antecedent and consequent, and determine to be consistent and equitable towards the two different kinds of uniformity in

question, we shall find that no theoretical distinction can be maintained between them. A man takes arsenic and dies; that was the 'cause' of his death, for the sequence is an invariable one, *if* no counteracting forces are at work. I kill a horse in a hot climate and it is eaten up by flies; this is only a 'uniformity,' but why? keep out the counteracting forces, and this sequence also will be invariable. Of course if any one beats off the flies the horse will not be devoured, but so if a surgeon comes with a stomach-pump the man may recover. We quite admit that in the one case the stages are many, and any of them can be interfered with; this is abundantly sufficient to establish an important practical distinction; but inasmuch as in the other case also interference is possible, no theoretical distinction can be based on this. If we shift our ground and claim the right to introduce a *C*, by declaring that the real cause and effect were not the arsenic and the death, but the arsenic and some intermediate change, we shall not secure the position; there would be the same necessity as before for excluding counteraction.

§ 10. The conclusion from the above investigation seems to be that unless we determine rigorously to adhere to an *immediate* antecedent we can never secure an unconditional and invariable one. It will not take much proof to show that such a determination would at once make the formula of universal causation barren and impracticable.

For what is an immediate antecedent? not the next link in the chain, for of links none are to be found in nature, but the next point in the strand of rope; and what is this? Take two points as near to one another as we please, another can always be interposed between them; examine any two stages in the sequence of phenomena, and any number more of intermediate stages can be conceived. The notion of an immediate antecedent can give us nothing, when strictly examined, but the tendency and magnitude of the forces in action at the point of time in question, but not the condition of things at any previous finite interval; it tells us only the form or law of development then and there, it does not give us successive stages of that development. To borrow a mathematical illustration, we can only determine, by means of this notion of immediate succession, the direction of a curved line at a given point, but we cannot discover any other point on the line however near to the given point.

§ 11. The notion then of invariable antecedence and consequence, when the antecedent and consequent are really immediate, seems to dwindle down to this;—Given the state of the phenomena at any given time, it declares the only phase of development which those phenomena can assume at that time, but it does not enable us to infer certainly what will be the condition of those phenomena after any finite interval,

however brief we may suppose that interval to be. Hence the barrenness of the formula; for telling us only what things are *becoming*, and not what they have been or will become, no real inferences about the future can be made by means of it. If B were the next link on a chain we could infer its presence from that of A by the law of causation, but if B be only an immediately neighbouring position on a strand that law will simply give us the *tendency* of things at A; we may thus give a tolerable guess as to whether B will follow or not, but we cannot certainly infer the fact.

§ 12. The general bearing of the last few sections upon the doctrine of causation seems to lead us to the following conclusion. In investigating nature, so far as we can without prepossessions, we come upon a large number of different successions which in their main features resemble one another. If we insist on introducing *all* the antecedents of any succession we must admit that the succession will be in almost every case unique, but there is a degree of determination short of this which enables us without appreciable error to speak of the same succession recurring repeatedly. When we do so speak, however, we find that the formula of Causation is still open to further and more serious objections. Each succession is composed of more or less of what are roughly called links of a chain. The more nearly in inti-

mate succession we suppose these links to be, the more nearly invariable does the succession tend as a general rule to become. This tendency thus observed over a certain extent, is greatly enlarged by Analogy, until we describe the *immediate* antecedent and consequent as forming an *invariable* unconditional succession. Of the doctrine, however, in this form, no use whatever can be made. For all practical purposes the cause must be understood in a sense in which the succession not being accurately immediate, is not really unconditional.

§ 13. So much then for the meaning of the term Causation. We seem led to the conclusion that it is an ambiguous term, having two senses, one an ideally precise, but almost useless sense, the other a rough sense adapted for working purposes. We shall now be better able to examine into the nature of the conflict, or supposed conflict, between this law, in either of the above senses, when it is generalized into a universal formula, and the assumptions which underlie the science of Probability.

§ 14. The following is the principal form which this conflict assumes. It has always been felt with more or less clearness that ignorance of the details, as combined with knowledge of the averages, is inseparably connected with the notion of probability. Hence arises anxiety in studying probability, lest the admission of this ignorance should be supposed to carry

along with it, as the ground of our ignorance, the assumption that the individual events in question can happen without causes. In most works upon the subject, therefore, whenever a discussion arises about our ignorance of particular events, whenever in fact the word *chance* has to be introduced, it is generally considered necessary to utter a caution against our believing in there really being such a thing as chance. Hume, for instance, in his short Essay on Probability, commences with the remark, "though there be no such thing as chance in the world, our ignorance of the real cause of any event has the same influence on the understanding &c." Such a caution as this has been especially insisted on by those who have written express treatises on Probability. I hardly know of one in which there is not inserted, somewhere at the outset, an emphatic disavowal of any belief in chance. Professor De Morgan goes so far as to declare that the foundations of the theory of Probability have ceased to exist in the mind that has formed the conception "that anything ever did happen or will happen without some particular reason why it should have been precisely what it was and not anything else." Somewhat similar remarks might be quoted from Laplace and others.

§ 15. The view above described refers principally to the natural and physical sciences. It there occupies rather a defensive position, the fact being insisted on

that whenever in these subjects we may happen to be ignorant of the details we have no warrant for assuming in consequence that the details are uncaused. But the corresponding view takes up a much more aggressive position when applied to social subjects. Here the attempt is often made to *prove* causation in the details from the known and admitted regularity in the averages. A considerable amount of controversy has been excited of recent years upon this topic, in great part owing to the vigorous and outspoken support of the Necessitarian side by Mr Buckle in his *History of Civilization.*

§ 16. It should be remarked that an attempt is sometimes made in these cases almost to startle the reader into acquiescence by the singularity of the examples chosen. Instances are selected which, though they possess no greater logical value, are, if one might so express it, *emotionally* more powerful. That the annual number of suicides should remain nearly the same is assumed to be strange enough, but what are we to say to the staggering fact that the number of misdirected letters annually sent to the post-office is about the same? Laplace himself scarcely dares to say more than that "he has heard that this is the case;" and writers of such repute as Dugald Stewart seem to have found real satisfaction in the fact that his assertion is after all only a hearsay.

§ 17. The aim of all such attempts is the same. It

is by the help of statistical uniformity to establish the existence of causation (in the sense of invariable unconditional sequence) in individual cases. I must confess, in spite of Professor De Morgan's assertion, that I cannot see that the matter, whichever way it be settled, has necessarily much to do with Probability. The caution no doubt, in the connection in which it generally occurs, may be a very useful one, for the opinions of the vulgar about the occurrence of events in games of chance is utterly vague and unscientific. But as a contribution to the theory of the subject I cannot help regarding it as needless, and even calculated to mislead. Our reason for employing the theory of Probability is our ignorance of the single events; but I cannot see that it is of the slightest importance from what cause this ignorance arises. It may be that ignorance is unavoidable from the nature of the case, there being no regular connection between antecedent and consequent; the causative link, as one may say, having been snapped. It may be that such a connection is known to exist, but that either we cannot discover it or that its discovery would be troublesome. It is the fact of this ignorance that makes us resort to the theory of Probability, the causes of it are quite irrelevant. When we do not know the events, considered singly, and choose our method just because we do not, it seems to me a mere digression to insist upon the fact that there is no

essential hindrance to our knowing them, and that we might do so were our faculties sharper than they are.

I am quite aware that on the view of Probability adopted by Professor De Morgan, the question assumes a somewhat different aspect. He, in common with many writers on the subject, seems to claim that the amount of our belief about the single event must admit of justification. My reasons for dissenting from this view have been already fully given; I need only therefore remark that if the view adopted in this Essay be correct we are absolved from any such justification, and are therefore perfectly indifferent as to what view may be taken about the single event. We are concerned only with averages, or with the single event as deduced from an average and conceived to form one of a series. We start with the assumption, grounded on experience, that there is uniformity in this average, and, so long as this is secured to us, we can afford to be perfectly indifferent to the fate, as regards causation, of the individuals which compose the average.

§ 18. When thus viewed the question to be decided assumes a rather different form. It can only be stated thus, Is the assumption mentioned in the last paragraph an impossible one under the circumstances? Or, by assuming that events of any kind display a uniformity in the long run, are we precluded from admitting that any or all of these events had no

regular antecedents or consequents? Let us take an example. We know from experience that when a penny is tossed up a great many times, heads and tails occur in about equal numbers. On the view now under discussion it is maintained to be quite essential that the result of each separate toss should have its invariable antecedent and consequent. I do not deny that this, as a distinct fact, may be true, but simply that it has any necessary connection with the previous assumption. For let us suppose that some or all of the throws had no invariable antecedents; what then? The fact of the general regularity being undeniable, the objector would have to assert that such a supposition was an impossible, or rather an inconsistent one. What is demanded is the proof by which he shows it to be inconsistent. This is surely no unreasonable demand, especially when we bear in mind the fact that the two doctrines, thus supposed to be inconsistent, have as a matter of fact constantly existed together in apparent harmony in the same minds at the same time. The harmony may be illogical, but if so, this should be distinctly proved. Millions, for instance, have believed in the uniformity of the seasons, who certainly did not believe in, and perhaps distinctly denied, the existence of necessary sequences in all the phenomena of each particular season.

§ 19. If we recur to the enquiry entered into in the earlier part of this chapter we shall find, I think,

that the conception of causation generally employed in these discussions fails in both of the respects to which attention was there drawn. As regards the introduction of *all* the antecedents into the cause, this is necessarily the case. Were this introduction insisted on, the sequence, as I have already remarked, would often be almost unique. Seldom or never should we be able to obtain enough instances to form statistics, except by neglecting a very considerable portion of the combination of antecedents. Statistics, from their nature, preclude any but a very slight degree of specialization. So also as regards the *immediateness* of the connection between cause and effect. So far from this being secured, the connexion in most statistical tables is of the loosest possible description. We there have set before us A's and B's with a very appreciable amount of separation between them. Nothing could be inferred in this way that would really bear upon so intimate a connection as that between the final form of the antecedent and the initial form of the consequent. The elements here under consideration are altogether disparate; we might as well attempt by reasoning about the separate links of a chain to draw conclusions about the molecular constitution of the iron, or discover whether the links were in actual physical contact or not.

It may be remarked that we are speaking here of what statistics might be conceived to be capable of

proving, not of what could be inferred from such tables as actually exist. These for the most part do not even offer anything that can be considered to be a sequence of *A*'s and *B*'s. Facts connected with one element only are laid before us, it may be thefts or murders or suicides. These actions, no doubt, may have their causes and their effects, but before a sequence of any kind, whether complete or incomplete, near or remote, can be inferred from such statistical tables, we must have other tables before us which shall refer to the supposed regular antecedents and consequents alike.

§ 20. It may fairly therefore be asked here whether the opinion that statistics have added to the extent over which causation can be shown to exist, is entirely incorrect. Briefly stated, my own view upon this subject is as follows;—It is only by means of causation that we are able as a general rule to make individual inferences about natural phenomena of any kind; that is, if we want to know what will happen under any circumstances we can only do this by ascertaining what has happened under the same or nearly similar circumstances before, and then making the assumption that the antecedents being the same the consequents also will be the same. In other words, inference is attainable, either actually or conceivably, wherever causation prevails, and not elsewhere.

Now what follows, as a natural consequence of

this, upon the discovery of statistical regularity? Take the instance of suicides. As regards the individual crime no certain inference whatever is possible. If the man's actions have their regular sequence of cause and effect most of the elements which combine to make any one antecedent are unknown to us. Hence the impression would not unnaturally arise that inferences would be equally unattainable in the case of the average. To give a numerical illustration, many persons would suppose that the numbers of suicides in successive years in London would present a series of about as great irregularity as the following;—1, 200, 50, 700, 3, 150 &c. For it is only through such irregularity that prediction would be precluded when we had a series of single events of this kind in contemplation. What we really find however is that the number remains about the same year by year. Hence inferences can be drawn, not indeed about individuals but about averages. And since in the former case, when the reference was to the individual instance, inference implied causation, it is assumed that the same must hold true in the latter case also.

§ 21. Of course if the meaning of causation be extended so as to include any kind of regularity whatever that enables us to make inferences, its existence can doubtless be proved by means of statistics; but this seems to be using the term in a sense very different from that of invariable unconditional sequence

with which we started. The popular ideas upon the subject would perhaps be expressed by saying that the regularity which is detected proves the events not to happen at random but to be under some kind of control. Turn such phrases as we may I cannot see more in them than a restatement of the fact that the observed regularity does exist; unless indeed the distinct error be involved that the regularity must somehow be produced by a kind of compulsion, so that causation in physical matters involves regularity produced by restraint. This would of course be to wander far from the scientific meaning of the term.

When we have observed the regularity in question for some time we are undoubtedly disposed to extend it further. If we find, for example, that a certain number of thefts have been committed each year for some time, we expect that the same state of things will continue for some time to come. In other words, we assume a certain degree of order or stability, in the operations of nature, and the statistics, if continued, confirm this assumption. As I have already said (Ch. VII. § 7), causation in this sense is certainly necessary for our inferences, and statistics continually prove its existence. But no amount of regularity of this kind seems to me to bring us nearer to proving that each separate event comprised in the statistics has its invariable and unconditional antecedents, which is the point at present in question.

§ 22. The proof of this latter fact should surely be sought, not in the regularity of the statistics, but in one sense in their irregularity. By finding that the number of thefts or suicides remains nearly the same we learn little or nothing about the question at issue; but if we found that every alteration in their number was connected with some alteration in such antecedent or concurrent circumstances, as the vigilance of the police, the happiness of the poor, or their political, moral, and religious progress, we should then begin to entertain a strong belief that some or all of these circumstances were causes of the observed and recorded phenomena. I only remark this in passing, to pursue it further would be to wander from our proper province.

§ 23. The mere regularity of the observed statistics, on the other hand, seems to me scarcely to have any connexion with causation, in the strict sense in which we have defined the term above, but to lead to an entirely distinct class of conclusions. It is in this way that the fact is ascertained, which was described and illustrated in the eleventh chapter, that almost all the properties of natural classes of objects preserve a general uniformity amidst individual variations. Thus, for example, although we cannot tell beforehand what may be the height, weight, &c. of any given man, we know that by continuing our observations over a sufficiently large extent we shall find these elements not only preserving tolerably nearly a certain average,

but grouping themselves in a regular and orderly way about this average. It is a very early result of observation to detect this regularity in the simplest properties and qualities of natural classes; what statistical uniformity seems to me to establish is, that a similar uniformity prevails in all the most recondite of these properties, and in almost all their remotest consequences. Take but one instance,—the observed fact that the number of misdirected letters remains about the same year by year. This appears to me to establish exactly the same kind of conclusions as the observed fact that the lengths of the lives of large bodies of men remain about the same. Just as observations of the latter kind show that the strength of constitution in different men preserves a tolerable regularity, so do those of the former kind show that there is a tolerable regularity in the strength of their memory. In each case the result is complicated by the cooperation of many external agencies to produce the observed result; statistics show also that these agencies themselves present a similar regularity.

§ 24. That which gives to discussions of this kind their chief interest is, no doubt, their supposed bearing upon the vexed question of the freedom of the will. I have no intention of entering further into this question than is necessarily involved by opposition to a line of proof which is often adopted in the discussion. I am, in fact, simply opposing an argu-

ment which is used against a particular doctrine; upon the independent truth of the doctrine itself I have no intention of expressing an opinion here. The connection between antecedent and consequent—in this case the determination of the will—would probably be universally admitted to be of the most intimate kind; the error of what for want of a better name must be called the school of Mr Buckle is, it appears to me, to attempt to prove a connection of this kind from statistics, which at best are only concerned with the less intimate connection of the events, that is, with the looser sense of the term. This refers, of course, to such inferences as may be drawn from the mere uniformity of the numbers of persons who perform certain acts, not (as already remarked) to a critical examination of the deviations from this uniformity.

§ 25. Before completing this chapter, I must make a brief reference to certain phases of some of the foregoing objections, which are theological rather than philosophical. They are however of no great present interest, since they are scarcely likely to be urged now in any form in which we can take notice of them here.

They are supposed inferences from Probability in favour of Atheism, and are of two kinds, which are distinct and indeed almost contradictory of one another. We will examine them in turn.

(1) An objection to Probability, which was once popular and which I suppose still lingers in some quarters, is that it refers events to *chance*. If we spelt this word with a capital C, and implied that it was representative of a distinct creative or administrative agency, we should, I presume, be guilty of some form of Manichæism. The only other meaning of the objection can be, that we assume that events (some events, that is) happen without a cause, and therefore remove them from the control of the Deity. But, as already pointed out, this is entirely a mistake. The science of Probability makes no assumption whatever about the way in which events are brought to pass, whether by causation or without it. It is simply a body of rules applicable to classes of cases in which we do not or cannot make inferences about the individuals; it commits itself to no opinion as to the ground of our inability to make such inferences. The objection therefore appears to amount to nothing more than this, that the assumptions upon which the science of Probability rests, and in consequence of which it is employed, are not inconsistent with a disbelief in causation within certain limits, causation of course being understood simply in the sense of regular sequence. So expressed the objection would (on my view) be perfectly true, though what bearing it could have upon Atheism is not easy to be seen, and must be left to any who urge it to explain.

§ 26. (2) The other objection is almost exactly the reverse of this, but is so utterly unreasonable that there is some difficulty in believing that the writers who have urged it really meant what they said. As it has been already alluded to in an extract from Laplace*, I will refer again to the example which he has there given. A man is supposed to have observed the fact that male and female births occur in the long run in a nearly constant ratio, and he has attributed this to Providence. All that need be meant by this is, that after he had observed the fact he came to the conclusion that the Creator had so arranged our frames that the births should occur on the average in this ratio. He then studies Probability, and ascertains that he had been labouring under a mistake, for that these proportions and numbers were in reality nothing but the 'development of the respective probabilities.' As I have stated before, if by the probabilities be meant the proportions, the assertion is tautological. If it be meant that there is something in our constitution by which the particular births are so brought about that on the average they occur equally often, the reply would of course be that this constitution, like everything else in our frame, had been produced mediately or immediately by our Creator. The physiologist believed already that there was some arrangement of our bodies by which each individual birth

* Chap. II. § 8.

was determined; unless the statistical data lead him by some means to detect this law, he knows absolutely nothing by the numbers that he did not know before.

The fact is that Probability has little more to do with Natural Theology (either for or against it) than the principles of Logic or Induction have. It is simply a body of rules for drawing inferences about classes of events which are distinguished by a certain quality. The believer in a Deity will, by the study of nature, be led to form an opinion about His works, and so to a certain extent about His attributes. But to propose that he should abandon this belief because the sequence of events,—not, observe, their tendency towards happiness or misery, good or evil,—is brought about in a way different from what he had expected, is so extraordinary a statement, that there is a difficulty in supposing that one has fully understood it.

§ 27. It is both amusing and instructive to consider what different feelings might have been produced in our minds by this connexion between, what may be called, individual ignorance and aggregate knowledge, had they presented themselves to our experience in a reverse order. Being utterly unable to make predictions about a single life, or the conduct of a single person, men are startled and sometimes terrified at the discovery that such predictions can be made when we are speaking of large numbers. And so some exclaim, This is denying Providence! It is

utter Fatalism! Now let us assume, for a moment, that our first acquaintance with the subject had been with the aggregate instead of the individual lives. We might conceive of something approaching to this in the case of an ignorant clerk in a Life Insurance Office, who had never thought of life except as having such a 'value' at such an age, and who had hardly dealt with it but in the form of averages. Can we not conceive his being astonished and dismayed when he first realized the utter uncertainty in which the single life is involved? And might not his exclamation be, in turn, Why this is denying Providence! It is utter Chaos and Chance! A belief in a Creator and Administrator of the world is not confined to any particular assumption about the sequence of events, but those who have been accustomed to regard events under one of the aspects above referred to, will often for a time feel at a loss how to connect them with the other.

CHAPTER XV.

ON STATISTICS AS APPLIED TO HUMAN ACTIONS.

§ 1. Throughout this Essay examples have been drawn almost indifferently both from purely physical phenomena and from those which are concerned directly with human actions; in the case of the latter, moreover, some of our examples refer to conduct which is purely voluntary, whilst in others the human will is but a remote cause of the effects described. It is now time to enter into a short enquiry as to how far it is right thus to put these voluntary actions upon the same footing as the results of the seasons or the turning up of the faces of a die, and to subject them all alike to the same rules.

The enquiry before us is, of course, but a limited portion of a far wider enquiry which has been much debated of late years, namely, whether what we have termed Phenomenalist or Material Logic is as applicable to the facts of society as it is generally admitted to be to those of inanimate nature. It is needless to say that nothing professing to be an adequate investigation of such a subject as this will be made in the present chapter; but the enquiry is one which must have been

so often suggested in some of the previous chapters, that it cannot be altogether passed over here. In Probability we are concerned only with a limited portion of human conduct, with actions, that is, which show some uniformity when arranged in a series, and with the inferences which can be drawn concerning them; but inasmuch as the criticisms which will presently be offered apply equally to drawing inferences about human actions of almost any kind, it will be simpler to commence our enquiry under the latter or more general form.

§ 2. It has been already repeatedly stated that the standing point occupied by the observer who is supposed to make the inferences we have been considering, is that in which he looks out on to things which are happening about him. He is supposed to observe coexistences and sequences of things around him, which he then proceeds to classify, and from which he draws what inferences he can. To retain such a standing point consistently two conditions, amongst others, seem to be presupposed. These are (1) That the observer should leave the things which he observes to work out their courses undisturbed by any interference on his own part. (2) That he should adhere consistently to the position of an observer, and not in imagination step down and take a place amongst the things which he observes. In the attempt to construct the Logic of Society, or Sociology as it is

often termed, both of the above conditions seem to me to be often neglected. The neglect of the former is, I think, an inherent imperfection in any such science of human conduct; that of the latter is rather a fallacy into which loose thinkers are apt to fall. We will examine these conditions in turn.

§ 3. (I.) * To say that the objects of any kind whose behaviour we are considering are to be left free from any interference on our own part, is to make a claim which is so obviously demanded, that the caution may seem unnecessary. And it certainly is not needed in the case of most inferences about inanimate objects. Any person can see that to draw inferences about a thing, and then to introduce a disturbance which was not contemplated when the inference was drawn, is to invalidate the results we have obtained. But when the inference is about the conduct of human beings it is often forgotten that in the inference itself, if published, we may have produced an unsuspected source of disturbance. In other words, if the results of our investigations be given in the form of statements as to what people are doing and what they will do, the moment these statements come before their notice the agents will be subject to a new motive which will produce a disturbance in the conduct which had been inferred. We may make what statements and criticisms

* The six sections which follow contain the substance of an article published by me in *Fraser's Magazine* for May 1862.

we please about the *past* conduct of men, but directly we commit ourselves to any statements about the future, or, in other words, begin to make predictions, we lay ourselves open to the difficulty just mentioned. That predictions can be made seems to be held by most of those who have adopted the application of logic now under consideration. They do not, of course, claim to be able to foretell the particular actions of individuals, but they constantly assert that it is quite possible that we may some day be able to foretell general tendencies, and the results of the conduct of large masses of men.

§ 4. The following extracts from Mr Mill's *Logic*, Bk. VI. ch. iii. § 2, will contain the best description of these claims of Sociology. After referring to the condition in which astronomy once was, and the science of the tides now is, he describes in the following words the practical aims of Sociology and the ideal perfection of the science, from which we are precluded only by the imperfection of our faculties:—"The science of human nature is of this description. It falls far short of the standard of exactness now realized in Astronomy; but there is no reason that it should not be as much a science as Tidology is, or as Astronomy was when its calculations had only mastered the main phenomena, but not the perturbations.

"The phenomena with which this science is conversant being the thoughts, feelings, and actions of human beings, it would have attained the ideal perfection of

a science if it enabled us to foretell how an individual would think, feel, or act, throughout life, with the same certainty with which astronomy enables us to predict the places and the occultations of the heavenly bodies."

§ 5. It will hardly be denied that there is the following distinct theoretical objection to the above illustration. The publication of the Nautical Almanack is not supposed to have the slightest effect upon the path of the planets, the publication of any prediction about the conduct of human beings (unless it were kept out of their sight, or expressed in unintelligible language) almost certainly would have some effect. The existence of this distinction renders all physical illustrations of any kind whatever entirely inapplicable when we thus attempt to explain the way in which it is supposed that human conduct can be studied and foretold.

§ 6. I wish it to be clearly understood that we are not here getting involved in any Fate and Free-will controversy; the objection before us does not arise out of the *foreknowledge*, but out of the *foretelling*, of what the agents are going to do. Assuming that the abstract possibility of foreseeing human conduct, alluded to in the extract above quoted, is quite compatible with our practical consciousness of freedom, I maintain that a difficulty of an entirely distinct character is introduced the moment we suppose that

this conduct is foretold, or rather, if one may use the term, *forepublished*. After all the causes have been estimated which can affect the agent, with the single exception of the sociological publication which describes his conduct, we shall perhaps find that the result is subsequently falsified by the disturbing agency of this publication itself.

This disturbance, observe, is not of the nature of a mere complication of the result; it takes the form of a distinct contradiction. Something was going to be done, and was therefore announced; in consequence of the announcement that thing is not done, but something else is done instead. But had this further consequence been foreseen (as we must, on our present assumption, suppose might have been the case) and allowed for, we still shall not find any escape from the difficulty. Were this all we had to take into account we should have nothing further to apprehend than a complication; but beyond all this there is the conflict between the final announcement and the conduct announced, which cannot be avoided. I repeat again, that it is not foreknowledge, but foretelling, that creates the difficulty; the observer, after he has made his announcement, or whilst he is making it, may be perfectly aware of the effect it will produce, and may even privately communicate the result to others, but once let him make it so public that it reaches the ears of those to whom it refers, and his work is undone. His position, in fact, is

somewhat like that of Jonah at Nineveh. Giving him the fullest recognition of his power of foreseeing things as they would actually happen, we must yet admit that he labours under an inherent incapacity of publicly announcing them in that form. The city was going to be destroyed; Jonah announces this; in consequence the people repent and are spared. But had he foretold their repentance and escape, the repentance might never have taken place. He might, of course, make a hypothetical statement, so as to provide for either alternative, but a categorical statement is always in danger of causing its own apparent falsification.

§ 7. The only reply, I think, can be that although the above difficulty is a theoretic objection, the effects referred to will be so utterly insignificant that they can be neglected in practice. But it is surely very doubtful whether distinct statements about human beings can be expected to produce little or no effect upon their conduct. The magnitude of this disturbing effect would seem to depend in great part upon the particular announcement made, and the intelligence of the agents referred to in it. If the announcement is concerned with matters of little importance, or with the conduct of persons who for any reason are not likely to take notice of what is said about them, then the considerations to which I have been referring might be neglected without serious error. We might calculate and publish as much as we

please about the fate of any of the depressed classes at the bottom of the social scale, without any serious anxiety that our predictions and conclusions would in consequence be falsified. But we should soon find the difference if we were to begin to discuss in this way the prospects of any persons who were likely to take an interest in our proceedings. It may be true that at present but little effect would be produced by any statements that we might publish about the future of society, because the possibility of making such statements is doubted; but if Sociology were ever to establish its claims, the effects produced in each case by its own disturbing agency would rise into real importance.

§ 8. The foregoing remarks apply principally to the case of a prediction being distinctly falsified owing to its statements being of a disagreeable character, but it must not be supposed that the difficulty is confined to such cases as these. The prediction may be equally falsified if it holds out an attractive prospect. There will not indeed be the same direct contradiction here, owing to the agents abstaining from what was foretold, but if, in consequence of the announcement, they perform their actions more speedily or more effectually than they would otherwise have done, the prediction is still rendered incorrect. The conduct, as it is finally carried out, is not the consequence of the motives which had been taken into account only, but of these together with the additional motives suggested by the pub-

lication. The nature of the disturbance which would be thus produced, as dependent upon the character of the announcement made and the circumstances under which it was published, have never, I think, been discussed, but seem well worthy of examination.

An instance has already occurred (Ch. VII. § 18) of the kind of practical contradiction which might arise from the cause now under discussion. The subject is far too wide for us to enter more fully into it here; I will only therefore call the reader's attention to the fact of the existence of this cause of disturbance and the consequent caution that any inferences from our statistics cannot be warranted, when we extend them into the future, unless under the condition that the persons whose conduct is described either are left in ignorance of the statistics, or, if they know them, are still uninfluenced by them.

§ 9. (II.) The remarks in the last few sections are intended to point out that that purely speculative and isolated position of the observer which alone is tenable when we are laying down rules for a science of inference, is one which it is in certain cases practically impossible to maintain. With every wish to be observers only we cannot always secure our isolation when we are describing the conduct of intelligent human beings, for we cannot always prevent them from being influenced by what we say. The criticism I have next to offer is of a very different kind. It refers

not to the actual disturbance caused unintentionally by the observer's published inferences, but to an intentional hypothetical disturbance in the actions which form the subject of the inference. The possibility of such a disturbance being contemplated arises from the fact that the observer himself, or other persons besides those to whom the inference refers, are themselves capable of acting in the same way as the persons whose conduct is described. Hence arise constant intrusions of the observer's personality into calculations from which they should be rigidly excluded. The point may seem somewhat subtle, and I must therefore bespeak the reader's attention to the following remarks.

§ 10. The statistics with which we are concerned in Probability are composed, as already stated, in great part of the voluntary actions of men. They may relate, for example, to crimes, such as the commonly adduced instances of murders, thefts, and suicides; to virtuous actions, such as the sums annually expended in charitable purposes; or to actions of an indifferent character, such as the number of marriages, or of insurances effected in the year. But of this portion of human conduct, as of most other portions, it is no matter of hypothesis, but a simple datum of experience, that in the long run, when we extend our observations over a sufficient space, a great degree of uniformity is generally observable.

§ 11. Now between statistics of this kind and

those which are concerned with what are not the immediate results of voluntary agency, whether the latter be of a purely involuntary character, as for example shipwrecks, or be results in which the human will is generally but a remote cause, as throws of dice, or births and deaths, there is one marked difference. It is this. We the observers, or any one else whom we suppose to occupy the position of observer, are ourselves beings like those whose conduct we tabulate and reason about, and the actions in question are such as we are or may be in the habit of performing ourselves. Hence it results that we are conceivably, if one may so say, a portion of our own statistics; we may suppose our own case to be included in the statistics under discussion. In many of the common examples taken from insurance, and above all in games of chance, the case is of course extremely different. There we may preserve with perfect consistency that purely Phenomenalist view in which we regard ourselves as looking passively on the successions of existences independent of ourselves. It would in fact be always difficult and often impossible not to take such a view there.

But though not impossible, it is exceedingly difficult to do the same when the things whose statistics we discuss are actions which men exactly like ourselves do perform, and which we any day may perform. To retain the correct view with rigid consistency it

would indeed be necessary to exclude ourselves entirely from the statistics, in other words to confine ourselves consistently to the observer's point of view, as we unavoidably do in the case of games of chance. We might help to compose the statistics of others, just as others compose the statistics for us, but we must not attempt to occupy both positions, those of observer and observed, simultaneously.

§ 12. I admit that owing to the peculiar character of the series of statistics of Probability, and the merely *average* truth with which we are there concerned, the inconsistent attempt just mentioned does not necessarily cause any error there. If indeed we were concerned with the absolute and universal statements of ordinary inference there would be error; the determination of a man, for example, to commit suicide when the inferential statement in which he was included had contemplated his abstaining from such an act, would falsify the inference. But no one man has power, by his own private conduct at least, to do much injury to an average. A registrar of deaths might drown himself as safely as any one else might, so far as affects the integrity of the formulæ with which he is concerned. His conduct is an isolated event, whereas the statistics continue indefinitely. Hence although his suicide is formally unwarranted, owing to the fact of its being an intrusion into statistics which had not contemplated it, it soon becomes over-

ruled, and its effects, even had they ever been perceptible, gradually vanish from sight. In the long run such a disturbance of course could not show itself, whilst in the individual instance, by one of the fundamental hypotheses of Probability (the irregularity of the details), we should not be justified in taking notice of the disturbance.

§ 13. It is not therefore exactly by this stepping down of the observer into the arena of the statistics, unwarranted as it is, that the fallacy now to be noticed arises. It is rather by certain hypothetical intrusions to which the acknowledged practical harmlessness of the actual intrusion gives rise, that error and confusion are caused. Finding that any one observer may without mischief do very much as he likes amongst the statistics, similar invasions are conceived upon such a scale as to involve the destruction of the speculative or scientific view, and, as we shall presently see, to cause amongst other things the expression of a great deal of practical fatalism.

§ 14. A quotation from Buckle's *History of Civilization* (Vol. I. p. 25) will form a convenient introduction to the discussion now to be entered upon. After pointing out that among public and registered crimes there is none which seems so completely dependent on the individual, and so little liable to interruption as suicide, he proceeds as follows:—"These being the peculiarities of this singular crime, it is surely an

astonishing fact, that all the evidence we possess respecting it points to one great conclusion, and can leave no doubt on our minds that suicide is merely the product of the general condition of society, and that the individual felon only carries into effect what is a necessary consequence of preceding circumstances. In a given state of society a certain number of persons must put an end to their own life*. This is the general law, and the special question as to who shall commit the crime depends of course upon special laws; which however, in their total action, must obey the large social law to which they are all subordinate. And the power of the larger law is so irresistible, that neither the love of life nor the fear of another world can avail anything towards even checking its operation."

§ 15. The above passage as it stands seems very absurd, and would I think, taken by itself, convey an extremely unfair opinion of its author's ability. But the views which it expresses are very prevalent, and are probably increasing with the spread of statistical information and study. They have moreover a still wider extension in the form of a vague sentiment than in that of a distinct doctrine. And as they are not likely to find a more intelligent and widely read expositor, or to be expressed in a more vigorous and outspoken way, I do not think I can do better than

*About 250 annually in London.

state my opinions in the form of a criticism upon this quotation.

§ 16. One portion of the quotation is plain enough. It simply asserts a statistical fact of the kind already familiar to us, namely, that about 250 persons annually commit suicide in London. This is all that the statistics themselves establish. But, secondly, this datum of experience is extended by Induction. The inference is drawn that about the same number of persons will continue for the future to commit suicide. Now this, though not lying within the strict ground of the science of Probability, is nevertheless a perfectly legitimate employment of Induction. The conclusion may or may not be correct as a matter of fact, but there can be no question that we are at liberty to extend our inferences beyond the strict ground of experience, and that the rules of inductive philosophy will furnish us with many directions for that purpose. We may admit therefore that, for some time to come, the annual number of suicides will in all likelihood continue to be about 250.

§ 17. But it will not take much trouble to show that there is a serious fallacy involved in most cases in the expression of such sentiments as those quoted. I am anxious that it should be clearly understood that this fallacy finds no countenance in either of the two assumptions which are necessary for the establishment respectively of the rules of Probability and Induction,

in those, namely, of statistical uniformity, and invariability of antecedence and sequence. In other words, the inference in the quotation would remain either unmeaning or false, in spite of our admitting that the number of persons who perform any assigned kind of action remains year by year about the same, and that the actions of each person are links in an invariable sequence*.

§ 18. We here have forced upon our attention the distinction between the two standing-points which may be occupied when we are investigating human conduct. Let us briefly examine them in turn.

(1) There is, firstly, that speculative point of view which, as I have said, we are in consistency bound to retain. On this view all these assertions about the inutility of efforts and the inefficiency of motives are meaningless, or rather inappropriate. What we are then discussing is, not what people might do if they were to resolve to alter their conduct, but what we

* It may prevent confusion if I remark here that my own opinion is in favour of Necessity, of the doctrine that is that where the antecedents are the same so will be the consequents, though I do not wish to speak with too great confidence. As stated above, such a doctrine is necessary for the establishment of strict rules of Induction, though not for that of Probability. The remarks in the last chapter were intended to show that whilst mere statistical uniformity scarcely had any bearing upon the question, it would not be easy by statistics of any kind rigidly to prove Necessity, and that when proved the doctrine would be comparatively barren of results as far as inferences are concerned.

have reason to infer that they *will* do. We are not concerned with the results of hypothetical alterations—these results might be of extreme importance—but we are drawing inferences as to whether such alterations will be made. All therefore that can be established by the fact of the statistical results remaining nearly the same is, that the amount of the counteracting efforts and the strength of the antagonist motives remain the same, not that these efforts and motives are in any sense ineffective. To prove this last point it would be necessary to take very different ground, namely, to examine instances in which such efforts had been made and instances in which they had not, and to show that the results in each case were nearly or exactly the same.

§ 19. (2) In distinction from the above there is the view taken from the practical standing-point. Every agent, whether or not his conduct form part of any table of statistics, finds himself the centre of a sphere of action. This view receives immense extension by each person being able to put himself in imagination into the position of any other individual, or into that of any body of individuals. When this position is occupied the question becomes a very different one from that last considered. We are not now considering whether efforts *will* be made, but we are distinctly taking into discussion the different results according as they are made or not. This would be the

most natural and appropriate explanation to be given of such remarks as those in the quotation before us, and could be the only one offered if we were referring to the efforts of a single individual or to those of a few people. All that any person could then mean by talking about the inutility of efforts would have no reference to the question whether efforts would really be made or not; he would simply mean that the difference, according as they were made or not, would be little or nothing.

It will scarcely be maintained, in this sense, that motives are feeble or efforts at suppression ineffective. Any considerable alteration in the belief of people as to a future world, or in their comfort in this world, would unquestionably have a great influence upon the number of murders or suicides. As regards the efforts of the clergy or magistrates to suppress the evil, however much these may be depreciated, it will not I apprehend be denied that a great deal might be done towards *increasing* the annual number of those who destroy themselves,—by removing the police, for instance, from the neighbourhood of the Serpentine and Waterloo Bridge. And it tells equally for our present argument, if it be admitted that the efforts of such persons could produce any consequence whatever, whether favourable or adverse.

§ 20. The reply to this would probably be, that though considerable consequences might really follow

were we to suppose an alteration in the conduct of persons in authority, or in the belief of the people, yet that we have no right to introduce such an imaginary alteration, because we know that as a matter of fact it will not really take place. This is probably quite true, and I have no intention of denying it; but what I wish to draw attention to, and what seems to be often overlooked, is how in such a reply as this we may be shifting from one point of view to another. We are abandoning the view taken in the last section and falling back upon the speculative view. When the efforts of a few persons are contemplated, the hypothesis of their acting otherwise is admitted, but the consequent effect is pronounced to be insignificant, as might very likely be the case. When however the efforts of many are contemplated, the hypothesis of their acting otherwise and the consequent effect, which would then be great, are not admitted, on the plea that they are inconsistent with fact.

§ 21. Such a confusion as that discussed above may seem absurd, but I cannot help thinking that in this way considerable support is often given to that practical fatalism which expresses itself in the common complaints about the utter impotence of the individual, and the irresistible power of great social laws, and which shows itself in our conduct by a selfish disposition to let everything good or evil take its own course without troubling ourselves about it. It is observed

that in the statistics of actions which may be the result, in their final form, of many different motives and of various conflicting struggles, there is yet year by year a marked uniformity. Instead of concluding from this, what alone ought to be concluded from the standing-point of a science of inference, that the motives and the efforts remain about the same year by year, a confusion is made between this and the practical view, and the doctrine is obtained that the efforts at repressing such conduct are unavailing.

§ 22. Such fatalistic views are often expressed in the form of disparaging comments upon the insignificance of individual efforts. In the sense in which this complaint is often made, I cannot but think that it is nothing more than an expression of our own selfishness, and really means, not that the results we could effect are small, but that we care little about them. Let us test this by taking some statistical uniformity, in which the motives that act to produce the result are almost entirely of a self-regarding character. Suppose that any one having ascertained that about a thousand persons, daily, in London, who could afford to eat a dinner, do for one reason or another go without it, were to announce this fact as a great social law of prodigious efficacy, and were to declare that its power was such, when examined on a large scale, that neither the fear of future hunger nor the love of good food could prevail towards even checking its operation, what would be

the natural reply? The form of these statistics and the nature of the argument grounded upon them are identical with those of the example cited by Mr Buckle. The reply would probably be, that if we were professing simply to draw inferences, most of this talk about the impossibility of checking such actions was, to say the least, inappropriate; but that if we were taking the practical view, that is, if we were for any purpose putting ourselves into the position of one or more of the individuals in question, the statement was utterly false. Each one of those men could in most cases have eaten his dinner or not according as he pleased, and therefore the whole body could have done so. And no sophistry about free-will and necessity would be allowed for a moment to stand in the way of such a judgment. Equally absurd would it be considered to talk about the insignificance of individual efforts in the face of such a great social law. But were people perfectly unselfish, statistics of crimes would not differ much from such an example as this. Almost any one individually might do something, and small bodies of men might do a great deal, towards diminishing crime, were it but in a single instance. When therefore it is vaguely complained that efforts are fruitless in the case of crimes, is there not some ground to think that the real meaning is that such efforts, on any much larger scale, are not likely to be made? And when it is urged that the individual can do nothing

in this case, whilst no one would dream of making the same assertion in the other case, are we wrong in thinking that the real difference is that the attainment of one's own dinner is more universally regarded as a substantial good than the suppression or diminution of our neighbour's faults? I do not, of course, deny that we should find it much more easy to dissuade people from some courses of conduct than from others; all that I mean is, that there is no real distinction between the general deductions which may be drawn from one or another kind of statistical regularity.

§ 23. A few remarks, chiefly by way of summary, may be offered in conclusion, inadequate as they are for the discussion of so important a subject as that treated of in this chapter.

We have been engaged, throughout this Essay, in considering the laws of inference applicable to the class of things which combine individual irregularity with aggregate regularity. Human actions of most kinds possess this property as unquestionably as do the throws of a die or the casualties caused by storms. The same laws of inference therefore which apply to the latter kind of events apply also to the former. But in saying this we are no more putting these two kinds of events upon the same footing than the historian is neglecting the distinction between virtue and vice when he employs the same rules of arithmetic to reckon up, say, the numbers who in any country

have been burnt at the stake as martyrs and hanged on the gallows as thieves. To say that our attention should, for purposes of inference only, be fixed upon one quality in an event, does not imply that we are to forget the existence of other qualities in that event. When we come to examine the actions to which the statistics refer, we find of course that they possess many other properties besides that which makes them fitting subjects of Probability. Their consequences may be of overwhelming importance, they may be the result of long deliberation and of bitter mental conflict, they may be morally admirable or detestable. But with all these latter qualities the logician, as such, is not concerned. His task is to make inferences. He has to calculate the chance of an event, whether that event be the throw of a die, a shipwreck, or a suicide.

Select almost any kind of action, and we shall find, if we extend our observation over a sufficiently large body of men, that there is a uniformity in the performance of that action. I have already called attention to some erroneous inferences which are often drawn from the existence of such uniformity. It was pointed out in the last chapter, that causation, in the individual instances, could not be proved in this way. It has also been repeatedly insisted on that to generalize as to the continuance of such uniformities beyond the limits within which they have been ac-

tually observed, though in many cases perfectly legitimate, belongs to Induction and not to Probability.

But at present we are not concerned with such considerations as these. We will assume that there is a long-continued uniformity in the frequency of the performance of some action, against which, it may be, large classes of persons are struggling with their whole strength. What we are now concerned with is the vital importance of the distinction between what may be called the speculative and the practical views which we may take in reference to any such uniformity.

What we have to adhere to, in making inferences, is the speculative view. On this view we have no right, when talking about the future, to use any other expressions than those which denote simple futurity. To say that the agents 'must' perform certain actions, or 'cannot' perform others, is inadmissible. To say this is to fall into a* fatalistic fallacy, for it generally involves a confusion between certainty of inference on our own part and compulsion on the part of the agents.

But against fatalism, in anything which has a close connexion with our own comfort or convenience,

* By Fatalism I understand the doctrine that events really dependent in part upon human agency, will yet be equally brought to pass whether men try to oppose or to forward them. It is essentially distinct from Necessity, and is indeed rather a sentiment than a doctrine, which it is difficult to state with brevity without making it obviously involve a contradiction.

the ordinary European mind is protected by a healthy instinct of incredulity. We should try in vain, by any effort, to persuade people that each agent could not generally alter his conduct if he pleased, or, consequently, that any body of men could not produce an appreciable effect if they were to try. The plain man feels that such statements as these are absurd; the thinker knows that, whichever way the doctrine of Necessity be settled, that doctrine does in no way whatever come into contact with the practical problems of life when stated in the above form.

When however the efforts of the agent are directed towards securing, not his own good but the good of others, the promptings of our natural indolence and selfishness offer dangerous facilities for the reception of such fatalistic doctrines, at least in the minds of those who are only looking on at the struggle and not sharing in it themselves. It is one thing to entertain the conviction that certain results will remain for some time uniform, because the conflicting efforts which produce them remain uniform. It is quite another thing to conclude that the efforts on one side are themselves ineffective. But the mere spectator, if his sympathies are not active, finds it only too easy to step across the logical chasm which separates one of these opinions from the other. I cannot help thinking that much support is thus given to the doctrine which one hears uttered in so many different forms,

and in every shade of dogmatism, by a certain school of writers, that the sorrows and the crimes of our fellow men are only the necessary product of the existing state of society, and that the efforts of the individual are insignificant. There are many perhaps who would have indolently told a Howard or a Wilberforce that he could do nothing, who would yet be very much astonished if asked whether the trouble of their own doctor in coming to see them produced insignificant results.

But the confusion between the speculative and the practical points of view produces, I think, still further consequences, quite as deplorable as those already described. Neither the Necessitarian nor the Fatalist need be men of blunted moral feelings. We might, that is, hate a wrong action though we thought it inevitable in the sense that the agent would not as a matter of fact choose to avoid committing it; we might even hate such an action though we thought it inevitable in the sense that the agent must do it whether he chose or not. But the complaint is often made, and I think not altogether unjustly, that the advocates of Sociology are too much in the habit of regarding crimes as being not only certain to happen, but as being morally indifferent. In so far as this complaint is true, I should think that such an apparent moral obtuseness of judgment (I shall not be misunderstood as hinting that

this is accompanied by moral laxity in practice) is connected with that confusion between two distinct views which has occupied our attention during this chapter. The connection would be as follows. The speculative view is in one sense wider than the practical, for the former includes not only voluntary actions, (the province of the practical view,) but also actions which are not voluntary, as well as results which are not strictly speaking actions at all, such as the faces turned up by dice. In the great majority of subjects to which this view introduces us, moral praise and blame have no applicability. When therefore the two views are confused together, we are sometimes apt, not merely to hamper our practice by fatalism, but even to run the risk of debasing our moral judgment by regarding the actions of men with the indifference with which we regard the happening of things. There is danger, for example, lest we should not merely believe that the number of murders or suicides are so fixed that efforts are unavailing to counteract them, but even that we should feel little more affected at the commission of crimes than at the successions of the throws of a die.

Against every such confusion between two views there is no safeguard comparable with that afforded by the habit of familiarizing ourselves with each view. In other words let us temper our speculations with a wholesome infusion of practice. Fatalism can-

not easily exist in the fresh air of practical life. The hardest workers are generally the most hopeful men, and in our own unselfish efforts will be found the best corrective to that depression which is so apt to be produced by a too exclusive devotion to the speculative view. We shall thus avoid the danger of always discussing the joys and the sorrows of our fellow-men in a way which, though legitimate when we are avowedly taking a partial view of the subject, too easily lapses into hopeless indifference or cynicism if we suffer ourselves to forget how partial that view is.

THE END.

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